THOMAS HARRIOT AND HIS WORLD

MATHEMATICS, EXPLORATION, AND NATURAL PHILOSOPHY IN EARLY MODERN ENGLAND

EDITED BY
ROBERT FOX

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Frontispiece Titlepage of *Admiranda narratio, fida tamen, de commodis et incolarvm ritibvs Virginiae ... Anglico scripta sermone à Thoma Hariot*, Latin edition of Harriot's *A briefe and true report of the new found land of Virginia*, published by Theodor de Bry (Frankfurt, 1590). By permission of the Rare Book Collection, J.Y. Joyner Library, East Carolina University, Greenville, NC, USA

Thomas Harriot and His World

Mathematics, Exploration, and Natural Philosophy in Early Modern England

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Dedicated to Max, Lord Egremont

With the gratitude of the Provost and Fellows of Oriel College, Oxford and of all students of Thomas Harriot

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Notes on Contributors

Pascal Brioist is Professor at the University François Rabelais, Tours and is a member of the university's Centre d'études supérieures de la Renaissance. From the time of his PhD at the European Institute on 'Intellectual circles in London from 1580 to 1680', he has been interested in Thomas Harriot's manuscripts, especially those concerning warfare. He is also a Leonardo da Vinci scholar and in 2010–11 was in charge of an exhibition on Leonardo's plan for a palace in Romorantin commissioned by François I.

Charles Fantazzi is Thomas Harriot Emeritus Distinguished Professor at East Carolina University. His chief research interests and writings are focused on Juan Luis Vives, Erasmus and Renaissance Latin poetry.

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Robert Goulding is Associate Professor in Liberal Studies and History and Philosophy of Science at the University of Notre Dame. He has presented papers at the Durham Thomas Harriot Seminar on Harriot's mathematics and optics, and is currently working on a book-length study of Harriot's optical manuscripts.

John Henry is Professor of the History of Science at the University of Edinburgh. Specializing in, amongst other things, the history of matter theory, and in particular in the atomic revival of the late Renaissance, his first engagement with Harriot was an attempt to assess the nature and extent of his atomism. Since then he has maintained an interest in Harriot and his contemporaries in English science, from Francis Bacon to Walter Warner.

Ian Maclean is Professor of Renaissance Studies at the University of Oxford and is a Senior Research Fellow of All Souls College. He has published widely on intellectual life in the late Renaissance and has held visiting professorships in Australia, Canada, France, Germany, the Netherlands and the USA.

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Mark Nicholls is Librarian and Tutor at St John's College, Cambridge and is former President of the College. He has published extensively on the court politics of late Elizabethan and early Stuart England, including studies of the Gunpowder Plot and the political career of the Ninth Earl of Northumberland, and a biography of Sir Walter Ralegh, written with Penry Williams.

Jon V. Pepper is an honorary research fellow in the mathematics department of University College London. In addition to that of Harriot, he is also interested in the work of Archimedes and in aspects of nineteenth-century mathematics.

Stephen Pumfrey is Senior Lecturer in the History of Science at Lancaster University. His research on practitioners such as Thomas Harriot, Edward Wright, Thomas Digges and above all William Gilbert has recently broadened into a study of science and patronage in Elizabethan and Jacobean England. He has been a regular participant in the Harriot Seminar and was proud to give Oriel College's Thomas Harriot Lecture in 2006.

Matthias Schemmel is a research scholar at the Max Planck Institute for the History of Science, Berlin, and is currently heading a research group on the long-term development of spatial concepts in the framework of the TOPOI Project Cluster. In his dissertation he reconstructed and analysed Harriot's work on projectile motion and the motion of fall. A detailed interpretation, including a facsimile and transcription of 180 folio pages of Harriot's notes, was published as *The English Galileo. Thomas Harriot's work on motion as an example of preclassical mechanics* (Dordrecht, 2008).

Jacqueline Stedall is Senior Research Fellow in History of Mathematics at the University of Oxford and is a Fellow of TheQueen's College, Oxford. She has written and lectured extensively on Harriot's mathematics. With Matthias Schemmel, she is currently preparing a digital edition of Harriot's manuscripts.

Diccon Swan paints portraits (of prime ministers, royalty, other luminaries and dogs). He was commissioned by the Provost to paint a replica for Oriel College of the Thomas Harriot portrait belonging to Trinity College, Oxford.

Larry E. Tise is Wilbur and Orville Wright Distinguished Professor of History in the Thomas Harriot College of Arts and Sciences at East Carolina University, where since 2004 he has conducted research into the life and career of Thomas Harriot and has arranged an annual Harriot Lecture for the University. In 2009 he organized an international conference on Thomas Harriot as astronomer, scientist, author, engineer and explorer, with sessions held in Chapel Hill, Raleigh, Greenville and Manteo in North Carolina.

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Preface

Oriel College's Thomas Harriot Lecture was inaugurated in 1990 and has been held annually at the College ever since. The Lecture recognizes Harriot's association with St Mary Hall, now part of Oriel, where he graduated in 1580. More broadly, it has allowed new light to be shed on Harriot's wide-ranging contributions to mathematics, the exploration of America and philosophical debate, as well as on his place in the political life of Elizabethan and post-Elizabethan England. Harriot was indeed a man of many worlds, and the present volume amply reflects that diversity.

The College's first 10 Harriot lecturers reworked their lectures for publication in *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), and revised versions of lectures, those delivered between 2000 and 2009, likewise form the core of *Thomas Harriot and his world. Mathematics, exploration, and natural philosophy in early modern England.* In the two decades since the lectures began, many debts have been incurred. Within the College, successive Provosts have provided enthusiastic support and it is a pleasure to acknowledge the part that the late Sir Zelman Cowan, the Revd Professor Ernest Nicholson and Sir Derek Morris have played in fostering Oriel's contribution to Harriot studies. It was Sir Derek's enthusiasm, for example, that resulted in the commissioning of the copy of the portrait in Trinity College, Oxford commonly, if somewhat uncertainly, supposed to be of Harriot. The copy, by Diccon Swan, is a fine one, and the work of copying proved unexpectedly illuminating, as Mr Swan shows in this volume.

The contributions to the volume by two colleagues from East Carolina University in Greenville, North Carolina are especially welcome. Charles Fantazzi's chapter on Harriot's Latin broaches a little-studied facet of Harriot's place in the Renaissance world of learning; from it Harriot emerges as an accomplished Latinist and Grecian. Larry E. Tise's study of the Theodor de Bry editions of Harriot's *A briefe and true report of the new found land of Virginia* grows from a long investigation that originated at the time when East Carolina University was engaged in purchasing a fine copy of a Latin de Bry. Both chapters arise from papers read at the Thomas Harriot quadricentennial conference that Professor Tise organized in North Carolina in April 2009. In the best possible way, this conference acknowledged the indefatigable enthusiasm for all aspects of Harriot's achievements on the part of a remarkable professor of English and former dean of the Thomas Harriot College of Arts and Sciences at East Carolina University, Keats Sparrow. It was a source of grief to all who knew him that Professor Sparrow died suddenly in November 2010. He had been instrumental in the naming of the College in 1994, and the appearance of this second volume of

Oriel lectures, which he awaited with characteristic enthusiasm, would have given him immense pleasure.

Colleagues who have followed the progress of the lectures will understand the debt that I owe to the contributors for the patience and care they have shown in helping to bring this collection of essays to fruition. In assembling their contributions for publication, I have been greatly helped by Dr Daniel Mitchell, who has also prepared a bibliography of works on Harriot that have appeared since 2000. Dr Mitchell's keen eye has made my task much easier than it might otherwise have been. It has been a privilege too to work once again with a publisher of exemplary proficiency and courtesy. At Ashgate, the passage to publication has been eased from start to finish by the support we have received from Dr John Smedley, whose scholarly interests and enthusiasm make any collaboration with him a pleasure, and his unfailingly efficient editorial colleagues Lianne Sherlock and Jon Lloyd.

Finally, the dedication of the volume to Lord Egremont conveys the gratitude of students of Harriot everywhere for the part he has played over many years in facilitating and promoting this and other ventures in the still expanding world of Harriot scholarship.

Robert Fox
Oxford
April 2012

Thomas Harriot Lectures 1990–2009

The following Thomas Harriot Lectures have been delivered at Oriel College since their inauguration in 1990.

David B. Quinn, 'Thomas Harriot and the problem of America', delivered on 7 May 1990.

Gordon R. Batho, 'Thomas Harriot and the Northumberland household', delivered on 20 May 1991.

Hugh Trevor-Roper, 'Harriot's physician: Theodore de Mayerne', delivered on 14 May 1992.

Hilary Gatti, 'The natural philosophy of Thomas Harriot', delivered on 20 May 1993.

Stephen Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', delivered on 19 May 1994.

J.A. Bennett, 'Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning', delivered on 18 May 1995.

Muriel Seltman, 'Harriot's algebra: reputation and reality', delivered on 2 May 1996.

John D. North, 'Stars and atoms', delivered on 22 May 1997.

John J. Roche, 'Harriot, Oxford, and twentieth-century historiography', delivered on 14 May 1998.

Scott Mandelbrote, 'The religion of Thomas Harriot', delivered on 20 May 1999.

Jon V. Pepper, 'Thomas Harriot and the great mathematical tradition', delivered on 11 May 2000.

Robert Goulding, 'Thomas Harriot's optical researches', delivered on 24 April 2001.

Jacqueline Stedall, '*The greate invention of algebra:* Thomas Harriot's treatise on equations', delivered on 16 May 2002.

Ian Maclean, 'Thomas Harriot on combinations', delivered on 22 May 2003.

Matthias Schemmel, 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', delivered on 10 June 2004.

John Henry, 'Why Thomas Harriot was not the English Galileo', delivered on 26 May 2005.

Stephen Pumfrey, 'Patronage, protection, and publication of scientists in the Renaissance: the strange case of Thomas Harriot', delivered on 18 May 2006.

Stephen Johnston, 'Thomas Harriot and the English experience of navigation', delivered on 17 May 2007.

Mark Nicholls, 'Last act? 1618 and the shaping of Sir Walter Raleigh's reputation', delivered on 22 May 2008.

Pascal Brioist, ‘Thomas Harriot and the worlds of practice: learning from seamen and soldiers’, delivered on 4 June 2009.

Since this list was compiled, Surekha Davies has given the 2011 Thomas Harriot Lecture on, ‘Thomas Harriot, John White, and the invention of the Algonquian Indian, 1585–1650’ on 19 May 2011.

The 2012 lecture, entitled ‘“The whole earth, a present for a Prince”: Molyneux’s English globes and the creation of a global vision in Harriot’s time’, is to be given by Lesley Cormack on 31 May 2012.

Revised versions of the lectures delivered between 1990 and 1999 were published in Robert Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot: Ashgate, 2000). Revised versions of the lectures delivered between 2000 and 2009, with the exception of Stephen Johnston’s lecture of 2007, appear in the present volume.

Introduction
The Many Worlds of Thomas Harriot

Robert Fox

In the words of the memorial plaque on his tomb in the church of St Christopher le Stocks in London, Thomas Harriot 'excelled in all things mathematical, philosophical, theological'. The praise was not exaggerated. Indeed, the list of Harriot's attainments could well have been extended. As a mathematician, he engaged with some of the leading problems of his age: in the theory of equations as well as the intensely practical world of ballistics. In optics, he arrived at the sine law of refraction more than 20 years before Willebrord Snell. As an astronomer, he developed a 'perspective glass', with his assistant Christopher Tooke, and is known to have used it to observe the moon in late July 1609, almost three weeks before Galileo presented his telescope to the Venetian Senate, and to have drawn a remarkably accurate map of the moon's surface.[1] In the history of exploration, he is remembered for his *Briefe and true report of the new found land of Virginia* (1588), the earliest detailed account of America in English, and for his remarkably accurate mapping of the coastal area of what is now North Carolina, close to where he landed on a voyage promoted by Sir Walter Ralegh in 1585. And along the way he moved, generally with ease though at times precariously, in the socially elevated circles of his two great patrons, first Ralegh and then, from the mid-1590s, Henry Percy, the ninth Earl of Northumberland, while also maintaining contact with some of the leading men of science in England and abroad.

For a man distinguished in so many worlds, the literary legacy that Harriot left behind is frustratingly sparse. Apart from the *Briefe and true report*, we have just one book that can be attributed to him, the posthumous *Artis analyticae praxis* (1631), a work on the theory of equations imperfectly compiled from a draft in Harriot's hand by Walter Warner, another member of the Northumberland circle, and Thomas Aylesbury, an executor of Harriot's will and a patron of mathematical learning. If historians had had to depend on these two sources alone, their understanding of Harriot would have been limited and, in important respects, distorted. But the disorderly collection of over 7,000 manuscript pages that Harriot left on his death in 1621 has served as a rich auxiliary quarry for historians ever since the German astronomer Baron Franz Xaver von Zach began examining the collection at Petworth House, a family home of the Percy family, in 1784.

[1] Stephen Pumfrey, 'Harriot's maps of the moon: new interpretations', *Notes and records of the Royal Society*, 63 (2009), 163–8, as well as Pumfrey's contribution to this volume.

Much of what we now know about Harriot's work has resulted from the study of these manuscripts, most of which were deposited in the British Museum in 1810. Extracting information from them has never been easy. But Harriot's distinction as a mathematician was quickly recognized by S.P. Rigaud, Oxford's Savilian Professor of Astronomy, who examined the mainly astronomical papers remaining at Petworth in 1832 and published an account of them in the following year.[2] For many years, by contrast, Harriot received less recognition for his description of Virginia until the Vermont book-dealer Henry N. Stevens reprinted the *Briefe and true report* and wrote the first book to be devoted to Harriot in 1900.[3]

Despite the lineage of admirers of Harriot's work, knowledge of his life remained fragmentary, as it does to this day. Details of his early life are particularly sketchy. We know only that he was born in Oxfordshire, probably in 1560, and that he entered St Mary Hall (an independent hall adjacent to Oriel College that was eventually assimilated as part of Oriel in 1902) as a plebeian member in 1576 or 1577. After taking a BA degree in 1580, he seems to have moved to London, where he began an association with Ralegh, probably in 1582 or 1583. By 1584, as a member of Ralegh's London household, he was teaching navigational and other techniques to the sea captains whom Ralegh had engaged for his proposed expeditions to the New World. It was at this time that Harriot wrote his 'Arcticon', an advanced treatise on navigation and cartography that he presented to Ralegh but never published. His departure for Virginia followed in the spring of 1585 (finally leaving Plymouth in May), and in July 1586 he arrived back in London, where he took up again with Ralegh. In the years that followed, he prospered both materially and intellectually. The patronage of Henry Percy maintained his prosperity and gave him, if anything, greater freedom. The annual pension that he received from Percy, known to be £80 in 1598 and £100 from 1616, as well as the lodgings he enjoyed for many years in or near Percy's residence at Syon House in Middlesex, provided the comfort and leisure that he needed for his scientific and other pursuits. His optical researches date from this period, as do some of his best mathematics and his excursions into alchemy, natural philosophy, and religion. In this later phase of his life, he had to tread carefully. Ralegh's trial for conspiring against James I in 1603 and the beginning of Percy's 16-year imprisonment in the Tower of London following the Gunpowder Plot in 1605 were unwelcome distractions that cast inevitable suspicion on anyone associated with either Ralegh or Percy. But, apart from the few weeks that he spent in the Gatehouse Prison in 1605 until he proved his innocence, Harriot's activities seem to have proceeded

[2] Stephen Peter Rigaud, *Supplement to Dr. Bradley's Miscellaneous Works, with an account of Harriot's astronomical papers* (Oxford, 1833).

[3] Thomas Harriot, *A briefe and true report of the new found land of Virginia, Sir Walter Raleigh's colony of MDLXXXV*, ed. Outis [Henry N. Stevens] (London, 1900) and Henry N. Stevens, *Thomas Hariot, the mathematician, the philosopher and the scholar. With ... biographical and bibliographical disquisitions upon the history of 'Ould Virginia'* (London, 1900).

unimpaired. When he finally showed signs of diminishing activity, as he did in his last years, the cause was almost certainly the painful cancer of the nose from which he died.

These are the bare bones of a life much as it was understood a century ago. But a resurgence of interest in Harriot, beginning in the 1950s, has transformed our appreciation of the work of someone whom Robert Goulding describes in this volume as 'the most accomplished early modern English scientist'. Among the pioneers of the revival, Cecily Tanner in England and John W. Shirley in the USA played leading roles. But they received enthusiastic support from David Quinn (with special reference to Harriot's exploration in America), Alistair Crombie, John North and John Roche (who together promoted regular Harriot seminars in Oxford from 1967 to 1983), and from Gordon Batho, who has chaired the annual Thomas Harriot Seminar in Durham since its inauguration by Tanner in 1977. The fruits of half a century of Harriot scholarship have been impressive. In the last decade alone, major books have included Jacqueline Stedall's critical reconstruction of Harriot's writings on algebraic equations,[4] a translation, with commentary, of the *Artis analyticae praxis*[5] and Matthias Schemmel's study of Harriot's place in the broader movement that led early modern science from the age of pre-classical to classical mechanics.[6]

And the work goes on, in the still-flourishing Durham Seminar, the Thomas Harriot lectures that have been given annually at Oriel College, Oxford since 1990, and in special events such as the international conference that took place at East Carolina University and other locations in North Carolina in April 2009, at the initiative of Professor Larry E. Tise.[7] Looking ahead, Jacqueline Stedall and Matthias Schemmel are laying plans for a digital edition of Harriot's manuscripts. High-resolution images of the British Library manuscripts have already been put online.[8] The next stage in this very welcome project will be to add transcripts, commentary and cross-referencing, thus creating an ambitious resource that will be permanently and freely available both to scholars and to the general public.

The contributions to this volume, all of them (apart from the appendices) arising from the Thomas Harriot lectures that have been delivered at Oriel since 2000,

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[4] Jacqueline Stedall, *The greate invention of algebra. Thomas Harriot's treatise on equations* (Oxford, 2003).

[5] Muriel Seltman and Robert Goulding (trans. and eds), *Thomas Harriot's Artis analyticae praxis. An English translation with commentary* (New York, 2007).

[6] Matthias Schemmel, *The English Galileo. Thomas Harriot's work on motion as an example of preclassical mechanics*, Boston studies in the philosophy of science, n. 268, 2 vols (London, 2008).

[7] 'Thomas Harriot quadricentennial conference', held in Greenville, Chapel Hill, and Raleigh, 1–4 April 2009. On the conference, which marked the 400th anniversary of Harriot's first observations with the telescope, see the East Carolina University website: http://www.ecu.edu/cs-cas/harriot400/conference.cfm.

[8] http://echo.mpiwg-berlin.mpg.de/content/scientific_revolution/harriot.

reflect the current vigour of research on Harriot and the diversity of perspectives that are attracting the attention of historians. A number of the contributions touch on Harriot's failure to publish more than he did. It is not only modern scholars who have been frustrated by this. Some of Harriot's friends too were disappointed, as Robert Goulding, Jacqueline Stedall and Stephen Pumfrey all point out. In a frequently quoted letter of February 1610, Sir William Lower warned Harriot that some of his 'inventions' risked being taken from him. The occasion for Lower's warning was the publication in the previous year of the *Astronomia nova*, in which Kepler had advanced his theory of elliptical planetary orbits. Whether Harriot had arrived at a similar idea is far from certain. But, according to Lower, he had at least rejected the idea that the orbits were perfect circles and had done so before reading Kepler's work. The consequences of what Lower saw as Harriot's undue reticence were plain. In the words of his admonishment, 'too much procrastination' risked depriving Harriot's 'country and friends' of the satisfaction of properly honouring his achievement.

The same point could have been made, and perhaps was made, with regard to other interests of Harriot. Stedall's contribution on Harriot's algebra, for example, shows how much can be gleaned from the manuscript sources she has studied, notably the papers of Nathaniel Torporley transferred from Sion College Library to Lambeth Palace Library in 1996 and virtually unknown until then.[9] As her reconstruction of Harriot's 'Treatise on equations' demonstrates, even when Warner and Aylesbury attempted to remedy matters, Harriot was ill-served by their inexpert presentation of his algebra in the *Praxis*. It is unsurprising that François Viète's two published treatises on the solving of equations, *De potestatum resolutione* (1600) and *De aequationum recognitione et emendatione* (1615), circulated more easily within the mathematical community and earned Viète a degree of celebrity among contemporaries that Harriot never enjoyed.[10]

Does it follow from this that Harriot's work in algebra passed unnoticed during and after his lifetime? Stedall argues that it does not. In the 30 years or so after Harriot's death, in an age when publication was not the necessary destiny of all scholarly endeavour, the work was known, respected and disseminated by a small circle of men of the stature of Charles Cavendish, John Wallis and John Pell. Times, though, were changing, as Pumfrey observes. Harriot's life, in fact, spanned an incipient transition in the learned world from a culture of communication founded on manuscripts to one founded on the printed word. The changes that were afoot were encapsulated in the contrast between the unhappy fate of the 'Arcticon', unpublished and in due course lost, and the success of a work on a very similar subject, the *Certaine errors of navigation* by the Cambridge graduate and Harriot's near-contemporary Edward Wright. Pumfrey observes that when the first of several

9 Stedall, *The greate invention of algebra*.

10 As Stedall points out, both works made available in print ideas that Viète had already elaborated in the early 1590s. See the introduction to Stedall, *The greate invention of algebra*, pp. 3–34, esp. pp. 6–7.

editions of *Certaine errors* was published in 1599, Harriot's failure to publish was the norm, while Wright's large volume was the exception. Yet there was more to the contrast between Harriot and Wright than a broader move to new ways of circulating knowledge, pertinent though that was. Pumfrey uses a discussion of the fine structure of patronage in Elizabethan England to demonstrate the inability or unreadiness of Harriot's two great patrons, Ralegh and Percy, to make publication a realistic option. He points to the positive merits of circulating work in manuscript form. When offered to a patron, a manuscript constituted a more intimate gift, free of the whiff of vulgarity that went with a commercial transaction or an unseemly quest for personal fame. The form also helped to protect the contents against uncontrolled diffusion in the entrepreneurial world in which many patrons lived. Why then did Wright's patron, the third Earl of Cumberland, authorize and even encourage the passage of *Certaine errors* into print? The explanation appears to lie in a complex web of charges and counter-charges of plagiarism, allied to Cumberland's position as a patron who could deliver a level of protection untouched by the suspicions that fed, to Harriot's detriment, on Ralegh's fall from grace at court and Percy's association with political and intellectual circles that made him a suspect figure in early Jacobean England. Hence, while the publication of Wright's book stands as an early exemplar of the gathering strength of the move towards print culture, Pumfrey also sees it as an initiative calling for the kind of micro-analysis he offers in his chapter.

John Henry similarly broaches the question of patronage in his contribution to the question of whether or not Harriot can be considered England's Galileo. Making his case against such a contention, he begins by contrasting Harriot's patrons, who were content with the practical advice and intellectual companionship that Harriot offered, with Cosimo II, who valued Galileo for the lustre that he brought to the Tuscan court at a time of intense cultural rivalry with competing city states. Cosimo's interest lay precisely in the high visibility that Galileo's work enjoyed. With these different priorities in mind, it is also relevant to Henry's argument that Harriot was first and foremost a mathematician and that he showed no aspiration to fame as a natural philosopher in the manner of Galileo. This is not to say that Harriot was indifferent to natural philosophy. However, his interest in the philosophical implications of his optics or his views on atomism seems to have been incidental rather than primary. As Henry argues, Harriot's overriding goal was to provide a detailed mathematical description of things rather than to investigate their causes. It was in just such a spirit that he meticulously recorded his observations with the telescope but, in an age when mathematics had yet to enter the heart of natural philosophy, did not pursue their implications.

Despite Harriot's reticence with regard both to publishing and to the more eye-catching aspects of his work, the circles of those able to comprehend his achievements were distinguished ones. His correspondence with Johannes Kepler, from which five letters written between 1606 and 1609 survive, illustrates the point. Starting from this correspondence, Robert Goulding explores aspects of a rather less self-effacing Harriot than we are used to. He points to a streak

of competitiveness in Harriot's exchanges with Kepler that led to deliberate obfuscation in what he said about refraction, a subject on which he evidently saw Kepler as a potential competitor. Goulding also presents a Harriot whose work on refraction had, as at least one of its objectives, the exploration of the innermost structure of matter. This brought his optical research close to the world of alchemy, another of the many intellectual worlds in which he moved. We know, for example, that he performed alchemical experiments for and with Percy in the Tower. It may well be then that, as Goulding argues, we should see Harriot's alchemy as a more central element in his experimental work, rather than the aberrant side-line that it was for John Shirley.[11]

Harriot's alchemical work raises the question of how far, if at all, he was beguiled by hermetic or occult thought or indeed whether 'scientific' and 'occult' mentalities can usefully be separated in the Renaissance. Ian Maclean insists that the occult had no place in Harriot's intellectual armoury. Harriot was certainly interested in the shape of numbers and in the kind of astronomical anagram that Galileo set for Kepler in 1610. But he does not appear to have seen any profounder mystical significance in combinations of either numbers or letters, or indeed in combinations of any other kind, for example, between the atoms of matter. Maclean associates his conclusion on this point with a warning against the temptation to interpret Harriot as engaging in his mathematical studies in pursuit of understanding in the realms of natural philosophy. Maclean's Harriot was eminently capable of 'compartmentalizing his mind'. Hence, as he argues, we should be ready to read much of Harriot's mathematical work straightforwardly as the explorations of a gifted mathematician working independently of the social, political or religious context that undoubtedly marked other of his writings, including not only the practical mathematics of navigation and cartography undertaken for Ralegh, but also hydrostatics and a variety of problems in Archimedean mechanics suggested by Percy.[12]

Even if we consider only the more abstract areas of Harriot's mathematical interests, the sheer variety, as well as the originality, of his work as a mathematician remains dazzling. Jon V. Pepper has no doubt on either count. On the grounds of both range and novelty, he places Harriot in a 'great mathematical tradition' studded with distinguished names, from Eudoxus and the mathematicians of antiquity to François Viète (Harriot's only contemporary rival in European mathematics) and on to Sir James Lighthill and others in our own times. Harriot's ingenious solution to the problem of determining the length of an equiangular spiral, the first known

[11] For a comment on Shirley's interpretation of the aberrant nature of Harriot's alchemy, see Stephen Clucas, 'Thomas Harriot and the field of knowledge in the Renaissance', in Robert Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 93–135 (pp. 105–6).

[12] On the sixteenth-century revival of interest in Archimedean mechanics and the related topics that Percy commended to Harriot and his other clients, see Clucas, 'Thomas Harriot and the field of knowledge', pp. 112–17.

rectification of any curved line (and that of the twisted loxodrome), the binomial theorem for fractional indices (another first) and its corollary, the exponential series (also a first), his 'triangular numbers' and their interpolational consequences, and his general importance in the mathematics of conformality all contribute to this evaluation. More generally, Pepper develops a portrayal of Harriot as an early and highly original contributor to the passage from geometric to symbolic algebraic formulations, a transition of which mathematics over the last four centuries has been the beneficiary. All this in addition to the new departures in notation for which Harriot has long been well known.

Other chapters in the volume reinforce Pepper's high evaluation of Harriot as a mathematician. In this vein, Jacqueline Stedall and Matthias Schemmel treat the very different mathematical realms of, respectively, algebra and mechanics. Stedall points to the problems that Harriot shared with Viète, whose work he knew well (initially through Torporley), and to important respects in which he went beyond the 'French Apollon' (the phrase is Torporley's). In her chapter, as more fully in her *The greate invention of algebra*, she shows how Harriot worked towards new methods of solving quadratic and other polynomial equations, starting from a set of 'canonical' equations with which any new equation could be compared and, by means of that comparison, solved. The result was, in Stedall's words, 'an original and beautiful piece of mathematics' that displayed Harriot's familiarity with Viète's work but also went significantly beyond Viète. The tragedy for Harriot was that the muddled posthumous rendering of his algebra in the *Praxis* did so much to cloud and so little to illuminate his contribution to the understanding of the structure of equations.

Schemmel uses his study of Harriot's mechanics not only to point to insights that were to become cornerstones of classical mechanics but also to explore the similarities between Harriot's work on the classic problems of projectile motion and the motion of falling bodies, and Galileo's handling of the same problems. The similarities bring out the striking extent of the shared body of empirical and theoretical knowledge on which both men could draw. As Schemmel maintains, this shared knowledge did not yet constitute a coherent framework of the kind that we now know as classical mechanics. Its elements were heterogeneous, ranging from aspects of Aristotelian physics to the intensely practical experience of gunners and engineers, and writings on mechanics in the period tended consequently to be eclectic. The insights of Harriot and Galileo were no exception. They reflected both the uncoordinated diversity of the sources on which they drew and the differences between the 'inferential pathways' by which they quite independently arrived at their results. Nevertheless, certain key results were identical. Both Harriot and Galileo, for example, arrived at the law of free fall. And, as Schemmel shows, their respective conceptualizations of projectile motion had much in common (not least the Aristotelian categories of natural and violent motion), even though in this case their results differed in significant respects. Broader generalizations are difficult. However, the evidence from this case suggests that disparate, though related lines of development in early modern mechanics were converging in such a way that the

long-term understanding of the science in question was largely unaffected by the dissimilarities we can identify between Harriot's and Galileo's approaches.

Schemmel's discussion of mechanics reminds us again of Harriot's capacity to move easily between his various worlds, in this case between the world of learned analyses of motion by Tartaglia or the Spanish artillerist Luys Collado and the intensely practical world of gunnery and warfare, given new immediacy by the heightened tensions between the European great powers. This 'real' world of soldiers, mariners and clashes between personal and national interests was never far from Harriot's experience. He knew it well from his association with both of his patrons, though with Ralegh in particular. And he knew what risks came with it. Mark Nicholls's examination of the trial that led to Ralegh's execution (in Harriot's presence) in 1618 reminds us how vulnerable a client could become when his patron fell from favour. By 1618, Harriot's active association with Ralegh lay in the distant past, yet depositions at the trial must have turned Harriot's thoughts to the vagaries of fortune that might well have had grave consequences for himself as well as for Ralegh. Nicholls convincingly interprets Ralegh's decision to take the path of candour and to give details of clandestine negotiations with members of the French court as a case of strategic political misjudgement. However, Ralegh's earlier conviction for treason (in 1603) had already left him, for 15 years, perilously vulnerable and under sentence of death.

The conviction of 1603 made Ralegh someone with whom Harriot could ill afford to be associated, even retrospectively. Despite Ralegh's fall, however, Harriot's debt to him remained profound. Ralegh had opened two new worlds to Harriot, and in that respect he stayed with Harriot long after Percy had replaced him as Harriot's main patron. One of the new worlds was America, with its Algonquian people and wealth of flora and fauna, all different from Harriot's European 'norms' yet, with an effort of interpretation, capable of assimilation to them: many of the plants and animals, though not identical to their European counterparts, were recognizable, and the demeanour, habits and religion of the people encouraged hopes of eventual cooperation between the native inhabitants and future colonists. Harriot's other new world was that of the mariners he encountered, through Ralegh, in London and on the voyage to Virginia. Pascal Brioist's chapter conveys the fascination that life on board held for Harriot. Using the evidence from manuscripts in the British Library, Brioist demonstrates the meticulousness of Harriot's observations of the practices of officers and their crew, the unfamiliar language they used and the fine details of rigging, knots and weaponry. As Brioist observes, the tone of the notes suggests that Harriot was observing and recording as an outsider looking in rather than as a full participant in the routines of life on board. The mariners' world was not his. Nevertheless, the notes reveal his remarkable capacity not only to assimilate and order unfamiliar practical information but also occasionally to conceive original solutions and to extend his discussion beyond simply what he had observed. Some of his notes on fortification and the disposition of pike-men in a battle-order reveal that facet of what Brioist properly describes as Harriot's 'restless intelligence'.

In this case, as in so many other aspects of Harriot's achievement, it is only rather recent scholarship that has revealed the full originality of his observations and insights. The thinness of the few printed sources and the voluminous but disorderly surviving manuscripts have presented historians with particular difficulties. And things might even have been worse. Only six copies of the original 1588 edition of the *Briefe and true report* have survived, a number so small that the flimsy unbound work might well have been lost had it not been for Richard Hakluyt's reprinting of the text in 1589[13] and the Latin, English, French, and German editions that the Flemish-born engraver and engraver Theodor de Bry published in Frankfurt in 1590.[14] Fortunately, the de Bry editions proved eminently saleable, largely because of the fine engravings that de Bry prepared from the watercolours by John White, who accompanied the voyage of 1585–86. But copies of even this relatively common work bear with them a diversity of histories. As Larry E. Tise recounts in his appendix to this volume, many of the 38 copies that he examined in detail had been interfered with, in most cases by Henry Stevens, who made the de Bry editions his speciality as a bookseller: pages had been trimmed, paper had often been washed or given gilt edges, and some of the copies had been made up and rebound using pages gathered from a variety of sources. Few copies were, in Tise's terminology, 'perfect'. One, however, came as close to perfection as could be hoped. This was the Latin edition that the J.Y. Joyner Library at East Carolina University bought in 2006.

It is particularly appropriate that a de Bry deemed 'perfect' should have turned out to be a copy of the Latin edition, since the commentaries on the engravings depicting life in Virginia were translated in the English, German and French editions from Harriot's original Latin. As Charles Fantazzi shows in his appendix, the Latin was elegant and the text was more carefully worked than the rather hurriedly composed English text of the *Briefe and true report*. In the ease and fluency of his Latin, Harriot showed himself to be a linguistically accomplished humanist, one whose immersion in the works of classical authors at Oxford also prepared him for distinction in yet another of his worlds, that of the literary coterie surrounding George Chapman. Within that circle, Harriot and his older friend Robert Hues (who graduated from St Mary Hall two years before him) appear

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[13] Richard Hakluyt, *The principall navigations, voiages and discoveries of the English nation, made by sea or over land, to the most remote and farthest distant quarters of the earth at any time within the compasse of these 1500 yeeres*, 3 vols (London, 1589), pp. 748–64; also in the later edition (London, 1598–1600).

[14] Thomas Harriot, *A briefe and true report of the new found land of Virginia, of the commodities and of the nature and manners of the naturall inhabitants. Discouered by the English colony there seated by Sir Richard Greinuile Knight in the yeere 1585* (Frankfurt, 1590). On the complex publishing history of the de Bry editions, see W. John Faupel, *A brief and true report of the new found land of Virginia. A study of the de Bry engravings* (East Grinstead, 1989), as well as Larry E. Tise's appendix to the present volume (Appendix A).

even to have had a hand in the work that culminated in Chapman's translations of Homer.[15]

Chapman's recognition of Harriot's command of Greek adds yet another dimension to the multi-faceted body of achievement for which Harriot was respected in his own day. What he contributed in any one of the worlds that he inhabited would have merited serious attention, but to have achieved distinction in so many of them marks Harriot as a figure of major importance in the later phases of the European as well as the English Renaissance. Seventy-five years ago, Frances Yates judged Harriot to be a crucial figure for our understanding of the intellectual movements of his age and one whose work cried out for systematic attention.[16] The authors of the chapters in the present volume would not dissent from such a view. Yates was right, and the gathering tide of Harriot scholarship bears witness to the seriousness with which her implicit challenge has been accepted, first by the pioneers to whom I referred earlier and now by younger generations of historians who continue to find in Harriot's work a quarry of inexhaustibly rich possibilities.

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[15] Chapman eloquently conveyed his admiration for Harriot in his lengthy poetic dedication to 'To my admired and soule-loved friend Mayster of all essentiall and true knowledge, M. Harriots', in Chapman, *Achilles shield translated as the other seven bookes of Homer, out of his eighteenth booke of Iliades* (London, 1598). For glowing acknowledgements to both Harriot and Hues, see also 'The preface to the reader' in Chapman, *The whole works of Homer prince of poets in his Iliads and Odysses* (London, 1616), unpaginated.

[16] See the comment by Yates quoted in Gordon R. Batho, 'Thomas Harriot's manuscripts', in Fox (ed.), *Thomas Harriot*, pp. 286–97 (p. 297).

Chapter 1
Thomas Harriot and the Great Mathematical Tradition

Jon V. Pepper

By now there can be few who are unaware of the many-sided interests, indeed the genius of the man who, just 420 years ago, graduated from St Mary Hall, now incorporated into Oriel College.[1] Thomas Harriot spent most of his life in the service of Sir Walter Ralegh and of Henry Percy, the ninth Earl of Northumberland, perhaps after a short period with Ralegh's step-brother, Sir Humphrey Gilbert, who was lost at sea in the autumn of 1583.

George Chapman's famous translations of Homer come with a fine poem addressed to Harriot (1598), whom Chapman praises as one 'whose judgement and knowledge in all kinds, I know to be incomparable' (1616).[2] It is interesting that the optical images in the verse of Chapman (who was by far the better poet) are echoed later in Halley's introductory Ode in Newton's *Principia* of 1687. Optics, a major field for Harriot and one that had always incorporated a mathematical component, was the medieval science *par excellence* and a key element in, for example, Dante's *Divina Commedia.* At the end of that work, Dante is vouchsafed a resolution of the quadrature of the circle, one of the great classical problems, and one in which Harriot had interests, both sceptical and speculative, as we shall see. But unfortunately Dante omitted to pass his enlightenment on to us in technical terms, and the world waited until C.L.F. Lindemann's paper of 1882 for a complete vision of the question, Bolyai having affirmatively solved the problem for his non-Euclidean geometry in the 1820s in certain cases.

Previous Harriot lecturers have told us about many areas of Harriot's interests and the intellectual ambience or climate of his place and times – and of course about the ever-fascinating, elevated and dangerous circles in or near which he moved. All of this is important, as indeed would be questions of his personality and character, and of his origins, if we could be certain of them. In this chapter, however, I want to address his mathematical work and his position in mathematics

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[1] Mine was the eleventh Thomas Harriot lecture. It was given not only in the millennium year 2000 but also in the centenary year of Cecily Tanner (1900–92), a great patron of Harriot studies and herself the author of many valuable papers. This text of the lecture is dedicated to the memory of Tom Whiteside (1932–2008) and John North (1934–2008). I am grateful to Dr Richard Povey for his help with the redrawing of the figures illustrating my chapter.

[2] G. Chapman, *Homer's Odysseys* (London, 1616).

(which is not quite the same thing), because this is where I believe he stands out from the numerous figures of his time. In some of this he is startlingly original, in other parts good and in the rest mostly pretty competent. As I shall argue, Harriot's numerous contributions fit into the continuing tradition of mathematics. One problem is that of publication. While Harriot published nothing in print other than his colonizing tract on Virginia (the North Carolina seaboard), knowledge of his earlier work in mathematics, astronomy and science was widely current in north-west Europe by about 1600.[3]

Harriot had specific connections with the Low Countries too: Sir Robert Sidney, later Viscount Lisle and Earl of Leicester, and a one-time Governor of Flushing, was an executor of Harriot's will in 1621. I have often thought that this, or some related connection, could explain Harriot's very early knowledge of the Dutch telescope, which led to some of his best astronomical work that has been described admirably by John Roche and others, and which I shall not discuss here, except to mention Harriot's early recognition that the theoretical merits of Copernicus's work were sometimes accompanied by a *lesser* accuracy in the resultant tables, presumably because of defective observational parameters.[4] As a related aside, I note that the sixteenth-century tomb of the priest Adam Harriot in the parish church of Kingston Lisle, west of here, might (if there were a family connection) help to explain Harriot's later circle of acquaintance.

Harriot's non-publication must be faced. While only conjecture, it is easy to see a number of possible reasons. First, as he progressed, his secure and generous patronage made publication less imperative. Secondly, the 'restricted' nature of some of his areas of work, to use modern parlance, made them rather like the 'classified work' of our days, not to be shared with others. Thirdly, there would be printing difficulties with much of it: the notations that Harriot developed were more suitable to manuscript work than the restricted linear presentation imposed by most printed texts (technical printing, then as now, being generally better in the Low Countries than here). Fourthly and related to this, there might have been doubts about the understanding and audience for such mathematical work, which would discourage a publisher and perhaps even a wealthy patron, and disappoint the author. Fifthly, and this might have been more relevant as Harriot aged, recurrences of ill-health, first mentioned in his petitions to Cecil in 1605, may offer some explanation. On top of all this, he was a busy man in both scientific and non-scientific ways. Perhaps, like Newton later on (*who* had decidedly non-publishing tendencies for much of his life), he might have feared controversies, as we see with his supposed religious views in the 1590s and later and his suggested Arminianism (Newton too had cause for reticence in similar matters).[5]

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[3] J.V. Pepper, 'The study of Thomas Harriot's manuscripts', *History of science*, 6 (1967), 33–6.

[4] J.J. Roche, 'Thomas Harriot's astronomy', DPhil thesis, University of Oxford, 1977.

[5] S. Mandelbrote, 'The religion of Thomas Harriot', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 246–79, esp. pp. 253–6; N. Tyacke,

So, for whatever reasons, Harriot did not publish to the world at large. However, he benefited from a wide reading of the works of others. To name but a few, he knew the current or recent work of such leading scholars as Rafael Bombelli (1526–72), whose *Algebra* had appeared in 1572, and the earlier *Ars magna* of Cardano (1545), whose algebra was still presented rhetorically; many works of the contemporary Belgian engineer and mathematician Simon Stevin (1548–1620); and the mathematics of his great near-contemporary François Viète (1540–1603), the father of modern mathematics, as the French, not unreasonably, still call him, and with whom there might have been some personal connection via the English mathematician Nathaniel Torporley (1564–1632).

One of Harriot's greatest results in science, the sine law of refraction, where in 1601 he anticipated Snell by 20 years and Descartes by 30 or more, was connected with his reading of Risner's 1571 Basle edition of Alhazen and Vitelo's *Optics*.[6] The copy he used has survived, with his own extensive notes and tables, and is now in the University of Oslo Library, to which it passed via the Danish Royal Library (how it got there no-one knows). Harriot was also familiar with Copernican and later Keplerian work, in the latter of which his friend and student, Sir William Lower (1570–1615), MP for Bodmin, later suggested his priority in general terms, though with what truth we do not know. Incidentally, it was Lower and not Harriot who saw merit in work of no value, rather than in no work, contrary to Ralph Staiger's popular text on Harriot.[7]

I mentioned earlier that we must separate Harriot's discoveries from his achievement. Let us first look at some of the former. His work in algebra, or at least some of it, *was* published, albeit in 1631, 10 years after his death. Strangely, this work has never been much admired, and John Wallis's championing of it against Descartes did not help either. In fact, despite omissions, Harriot's *Praxis* is a model of logic and organization compared with Descartes's brilliantly allusive work of 1637. Muriel Seltman has recently taken this work up again, so I will trespass only a little on it now.[8] Here (and in his manuscripts), Harriot extends the algebraic notations of Viète almost to the modern elementary form, including a printer's version of his inequality signs (still in use) and he extends numerical methods of solution, precursors of the modern methods of Newton and others later in the century.[9] Historians of mathematics are at heart pure mathematicians and have the concomitant prejudices against applications and numerical methods, as

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'Puritanism, Arminianism, and counter-revolution', in R. Cust and A. Hughes (eds), *The English Civil War* (London, 1997), pp. 136–59.

6 J.A. Lohne, 'Thomas Harriot (1560–1621), the Tycho Brahe of optics', *Centaurus*, 11 (1965), 19–45.

7 R.C. Staiger, *Thomas Harriot. Science pioneer* (New York, 1998).

8 M. Seltman, 'Harriot's algebra: reputation and reality', in Fox (ed.), *Thomas Harriot*, pp. 153–85.

9 J. Stedall, *The greate invention of algebra. Thomas Harriot's Treatise on equations* (Oxford, 2003).

one sees, for example, in many modern accounts of Archimedes's *Measurement of the circle*, with their dismissal of the work in Proposition three, or travesties when it is discussed. However, for the pure mathematician, Harriot offers the relating of roots to the binomial factors of polynomials, obvious we might think going one way, but hard in its converse.[10] As a bonus, Harriot obtains a number of algebraic inequalities, and his arguments on the number of positive roots are models of logical presentation. Above all, the work is profound for its development and use of algebraic symbolisms to the exclusion of earlier methods, and for a largely non-verbal text. Of course, we do not know to what extent Harriot's own presentation might have been similar, but much if not all of his surviving manuscript material is equally terse.

The word 'algebra' is an ambiguous one, originating in equation-handling in the Arab period, when symbolisms do *not* occur, Diophantus's earlier signs having been long forgotten. After Viète, Harriot and Descartes, it increasingly meant the algebraic symbolism applied to the earlier algebra and (with Harriot) to much else as well: geometry, numerical work, mechanics, optics, ballistics, cartography and so on. Who said that co-ordinate geometry was the invention of Descartes, or perhaps Fermat? It appears, as one would expect, at the same time as algebraic symbolisms, that is, with Viète and Harriot. Like Viète, Harriot solved a number of interesting problems with the new algebraic symbolisms. But, again like Viète, the importance of this is probably more in the symbolisms and their future versatility than in the specific individual problems and their solutions. It is easier to see this now than at any time since the seventeenth century. In other words, we see *methods* rather than results, the wood rather than the trees. The great achievement of seventeenth-century algebra was arguably in the calculus varieties of Newton and Leibniz. This was later, and the continuing development of algebra, via Descartes and Newton, made key contributions.

But some of these results were to recur. These include the interpolation and area methods, rediscovered later by Newton, Gregory and Simpson, and, related to these, the binomial theorem for fractional indices that Harriot derived and used long before Newton. He did this by reversing the time-honoured methods of tabulators, who obtained the differences from the tabular values. Harriot saw that these values could be constructed from successions of differences. Henry Briggs did something similar a little later and described it in the preface to his 1624 tables of logarithms. It is ingenious and very useful, but perhaps fairly straightforward. No-one since Duncan Fraser (1927) has believed, against the evidence, that Newton read this preface before his discoveries of the mid-1660s; in any case, Newton was solving a rather different interpolation problem.[11] Mathematicians are often criticized for seeing history (when they notice it at all) in terms of priorities, and indeed many of Harriot's results are examples of discoveries repeated in subsequent years,

[10] H.M. Edwards, *Galois theory* (New York, 1984); I. Stewart, *Galois theory* (London, 1989).

[11] D.C. Fraser, *Newton's interpolation formulas* (London, 1927).

where connections are unclear. But honour where honour is due. No-one should be ashamed to call Harriot the discoverer of the exponential series, which arose from his binomial theorem via a limiting calculation of interest instant by instant (as Lohne noticed long ago).[12]

Again, most of us would be glad to know that we had discovered the sine law of refraction and applied it to questions of dispersion and the height of the primary rainbow, in the last case probably via some restricted turning value method, obtainable later by elementary calculus. The primary rainbow occurs when $\tan i = 2\tan r$, where i is the angle of incidence and r is the angle of refraction, as Harriot states and we can now easily show. Some years ago, presenting this to a not undistinguished audience, I met a comment at the end: 'but he did not have an overall theory of light', implying (I suppose) that he had 'merely' made a lucky strike. But this was wrong: the reasoning before the result *was* a conceptual framework, and the subsequent experimental verifications completed a respectable model for science.

Such a theory of light – atomistic or, as we might say later, corpuscular – could have been behind his important theory of impacts, imperfect because of a certain lack of symmetry, odd for such a fluent algebraist, but presented in an elegant algebra. Attempts to see this in the light of later ideas of energy and momentum are, I believe, misguided. The links are closer to late medieval ideas that were well known in the sixteenth century. The same mistake was made several years ago in reading Newton anachronistically.[13]

Harriot was, beyond all this, an excellent calculator. The well-known British Library print with the poem by his friend George Chapman, dated 1620 and published in 1940, while not accepted by everyone as definitely of Harriot, seems to me to be beyond reasonable doubt.[14] It was once said to be of Napier, but did *he* know Chapman? And he was dead by 1617. John North has identified the mathematical instrument shown in it as Blagrave's Jewel (published 1585). Blagrave himself lived near Reading, close to where Harriot had family connections, as we learn from his will. When Harriot calculated his great tables of meridional parts, finished by 1614 and unequalled until the 1920s, he had the assistance of his servant Christopher Tooke, who must receive a mention in any dispatches on this area of work or indeed of Harriot's observational astronomy.

This work often comes under the rubric of navigation. England is of course a maritime nation, but somehow navigational science and mathematics have often

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12 J.A. Lohne, 'Dokumente zur Revalidierung von Thomas Harriot als Algebraiker', *Archive for history of exact sciences*, 3 (1966), 185–205.

13 See, for example, S. Chandrasekhar, *Newton's Principia for the common reader* (Oxford, 1995).

14 J. Robertson, 'Some additional poems of George Chapman', *The library*, 4th ser. 22 (1941), 168–76.

been treated as rather secondary, not to say second-rate.[15] In the old days, by which I mean before the computer age, this meant a good knowledge of spherical trigonometry and high competence in the use of arithmetical logarithms (which Evariste Galois [1811–32] told his Ecole polytechnique examiners did not exist, a truth with tragic consequences). However good one's knowledge here, it was not seen to make one a good mathematician, rather the reverse in most people's view. But in his tabular work, Harriot was at least in the great traditions of Ptolemy (and perhaps Hipparchus before him), of the great Arab astronomers, of the fifteenth-century Regiomontanus, and of Rhaeticus and Valentine Otto in his own century. In fact, in terms of his theoretical basis, he went well beyond them, just as Newton went beyond Kepler, and Kepler surpassed Copernicus. One of Harriot's most notable achievements was the direct calculation of the extended meridian lines necessary to construct a Mercator mapping, that is, one in which *angles* are preserved, so that a course may be read off directly. The problem had been published by Mercator in 1569 (following earlier hints) but was not solved by him. Leaving aside the complex details of Harriot's solution, what is important here are the methods and results that he devised on the route to that mapping. These are the conformality of stereographic projection, the rectifications, the interpolation methods and the binomial theorem.

In 1637 Descartes denied that curved lines had determinable lengths: not just that we cannot find them because we are not yet clever enough, but that it was beyond human knowledge. After all, we might say, nearly four centuries later, that this need not be surprising. There is no evolutionary gain in knowing such things, if they are knowable. But Harriot had already refuted this view or at least bypassed it by a suitable implicit definition, when in connection with the Mercator or meridional parts question, he rectified (as it is called) not only the plane equiangular spiral (later also known as the logarithmic spiral, and distinct from the spirals of Archimedes and of Fermat) but also the twisted loxodromic curve on the sphere. There is in fact a simple and perhaps unexpected relationship between them, as in this case (but not always in this area of problems) the plane triangle approximation turns out to be exact.

Harriot had three methods. They were separately algebraic, geometric and arithmetical, and involved double limits (Figure 1.1). A hundred years later, the elder Bernoulli obtained the plane result by simple calculus. Less well known, perhaps, are Newton's results in *Principia*, Book II, section four, on so-called circular motion in resisting media, Newton's circles being in fact equiangular spirals, a possible orbit for a descending earth satellite, something rediscovered by Dr King-Hele and others at Farnborough in the 1950s and 1960s. In retrospect, because of its similarity properties (I suppose we would say 'fractal' these days), this spiral is perhaps the curve one would expect to be solved first.

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[15] J.E. Littlewood, *A mathematician's miscellany* (London, 1953). For exceptions to this generalization, see J.A. Bennett, 'Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning', in Fox (ed.), *Thomas Harriot*, pp. 137–52.

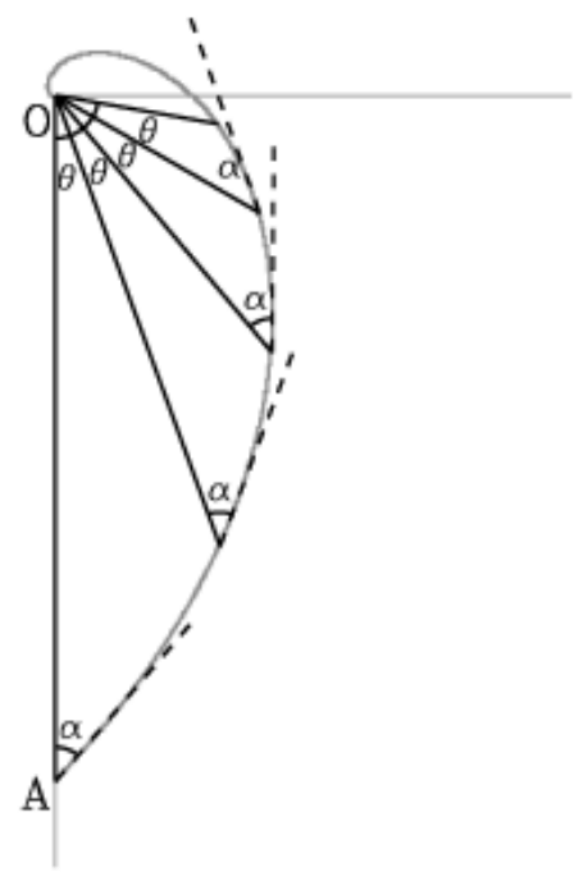

Figure 1.1 Equiangular spiral with angle α and an infinite number of turns near origin O

No-one before Harriot had found the exact length of *any* curve: neither Eudoxos nor Archimedes, none of the Indians or Arabs, subtle as they were, nor even the Englishman Richard of Wallingford, nor the fourteenth-century Frenchman Nicole Oresme. Archimedes had found the *area* of the parabolic sector, a result which, when expressed in terms of differences, Harriot used in his tabular work. Archimedes had at least three different methods for this area. But its length, if he ever sought it, eluded him, for reasons that are now clear to us. It was, in post-Napierian speech, logarithmic, which is the same as hyperbolic, though this identity seems not to have been noticed generally until the 1640s. Torricelli had Harriot's plane spiral result again in the 1640s, but it was not until the 1650s that Hendrik van Heuraet (1633–60?) and William Neile (1637–70) independently rectified the semi-cubical parabola, the first rectification of an *algebraic* curve. Christopher Wren with great ingenuity did the same for the cycloid, an early sixteenth-century French curve, as far as we know, Roberval having previously found its area by Cavalierian infinitesimals. Van Heuraet's method (published in 1659 in van Schooten's second Latin edition of Descartes's *Geometria*, a work from which both Newton and Leibniz benefited greatly)[16] was apparently a special case, or a special method for a special curve, but it is now seen to be an early generalizable example of the so-called fundamental theorem of the calculus or, in clearer terms, the inverse nature of tangents and areas, seen a little later, in various ways, by Newton, James Gregory, Isaac Barrow, Leibniz and perhaps others. Van Heuraet also gave the related parabolic rectification.[17]

16 *Geometria. À Renato Des Cartes anno 1637 Gallicè edita, postea autem unà cum notis Florimondi de Beaune* (Amsterdam, 1659).

17 J. Fauvel and J. Gray (eds), *The history of mathematics. A reader* (Basingstoke, 1987), pp. 354–6, 610.

There are technical reasons, backed up by various paradoxes, why length is a trickier idea than area. As such, it is not surprising that precise lengths appear much later than areas, though in the case of the circle Archimedes gave a simple relationship between the two properties. These questions exercised many in the 1650s, including those already mentioned, as well as Huygens, Pascal, Sluse, Fermat and others. Harriot had related the plane spiral to the loxodrome (or course of constant bearing) on the sphere or globe, using the so-called stereographic projection, which he proved (perhaps for the first time) to be conformal, that is, angle-preserving. This turned an awkward problem on the sphere into a plane problem involving an everywhere similar plane curve, one incidentally whose limiting case is a circle. (In modern terms, in this problem he is able to replace the integration of the secant function by the rectification of the spiral.) This last property is not so helpful as it might be, as it turns out in this case that properties true *up* to the limit do not hold *in* the limit, something mathematicians have since become used to, following a well-known series of such results produced by the great Norwegian mathematician Niels Abel (1802–29) in the 1820s. Here it precludes the rectification of the spiral from implying that of the circle, though Harriot had hoped that something might be made of this. He had already annoyed J.J. Scaliger, the great classical scholar, by refuting the latter's claimed quadrature of the circle and solution of other claimed classical problems. Viète had no more thanks for his similar maintenance of mathematical veracity on the same issue; the question reminds one of the later dispute between Hobbes and Wallis on the same matters. There are in fact many very accurate constructive solutions of this and similar problems by Euclidean methods. But none is or can be exact, although this was not finally proved until the nineteenth century.

It is important to note that Harriot's conformal property was not known in the general case to Ptolemy, as is sometimes stated; this is based on a misunderstanding.[18] Harriot might have been the first to declare it. However, as North has shown, it was a result that could not have been overlooked by later astrolabe makers. If I were lecturing in a mathematics department, I would have started with a definition – that is often how they do things – as follows:

> Def'n: A mathematical lecture is a lecture in which at least one non-trivial result is given a decent proof.

Here, a few diagrams may help. Harriot's standard picture of his spiral is shown in Figure 1.1. The 'equiangular' epithet means that the curve crosses the so-called radius vectors at a constant angle, here α. In one part of his work, Harriot approximates this by a series of triangles, which, because of the constant angle, are all similar, that is, the corresponding angles are equal (Figure 1.2).[19] Hence,

[18] H. Michel, *Traité de l'astrolabe* (Paris, 1947), pp. 20–21.

[19] Figure 1.2 recalls the diagrams of Newton's propositions 1 and 2 in Book I of the *Principia*, where he shows quite possibly (the argument is amazingly convincing but may

by Euclid VI.4, their sides are proportional, and the lengths AB, BC, CD and so on form a geometric progression, whose sum Harriot knew. (Fifty years later, in 1647, Gregory of St Vincent could still take many pages to get this result in his *Opus geometricum*; in fact, the question occupied most of Book II of what is a spectacularly long work.)

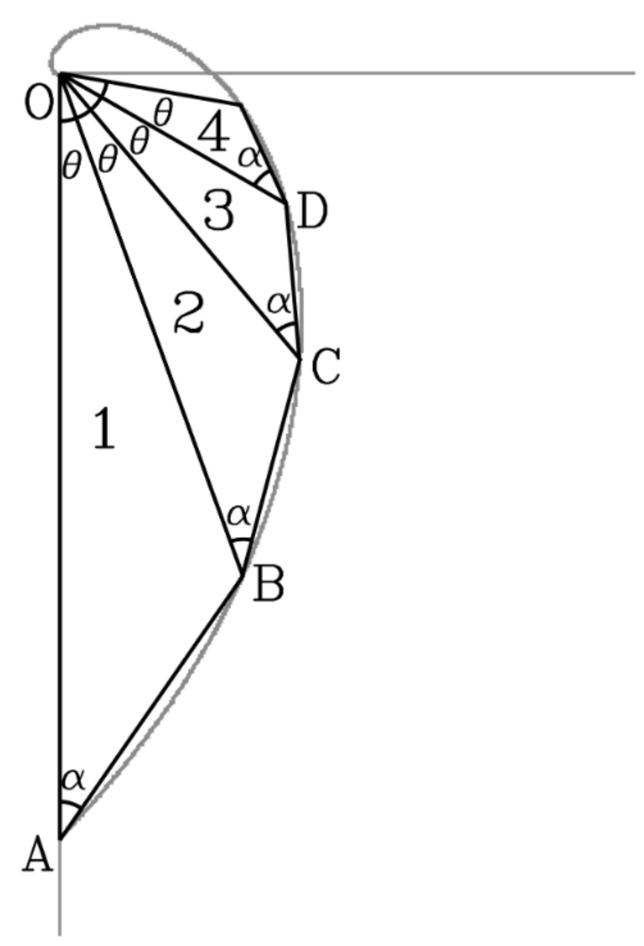

Figure 1.2 Approximately polygonal curves. Triangles AOB, BOC, COD and so on are similar and hence the sides are proportional

By also letting the equal angles AÔB, BÔC, CÔD and so on tend to zero, Harriot found the limiting length and hence (assuming such things 'exist') he rectified the spiral. In fact, the result is very easy to state. The total length, involving an infinite number of turns, is simply OA times the secant of the angle α; and this is just the length Aβ, where Aβ is tangent to the curve at A and AÔβ is a right angle (Figure 1.3). To find the length of the segment of the spiral, just replace OA by OA minus OA′, where A and A′ are the ends of the segments. The spiral has no memory and in a sense is the same everywhere, except for scale. This means that it has a curvature proportional to the radius vector, a result Newton may have assumed in *Principia* Book I, section 2, proposition 9, where he finds that the motion in such a spiral arises from an inverse *cube* central force. In the limiting case where α is a right angle, we get a circle, but as I have already indicated, the spiral results do not help with the corresponding circle results.

In a series of diagrams, Harriot in one of his treatments fits the approximating triangles alternately into the right-angled triangle OAβ, or part of it, namely OAE (Figure 1.4). It is then easy to see that the *length* will be the limit of the sum of AE and OE as the angles AÔB and so on all tend to zero. This is again Aβ, namely OAsecα. The *area* will be the limit of the area OAE, namely ½.½tanα.OA^2, that is,

conceal unsuspected subtleties) that the central orbit and the equal areas properties of orbits are equivalent.

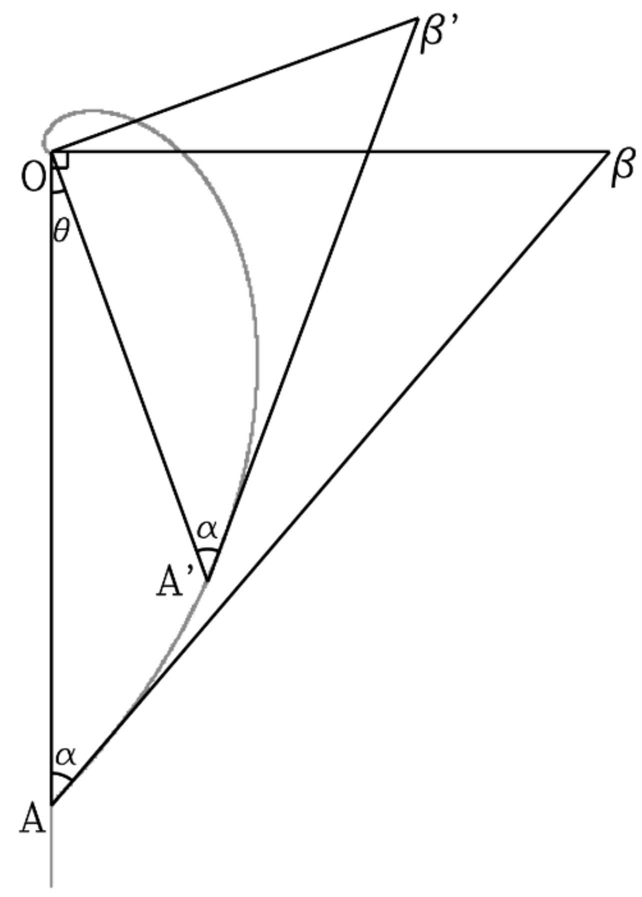

Figure 1.3 Total length of spiral = Aβ = OAsecα. Length AA′ = Aβ – A′β′

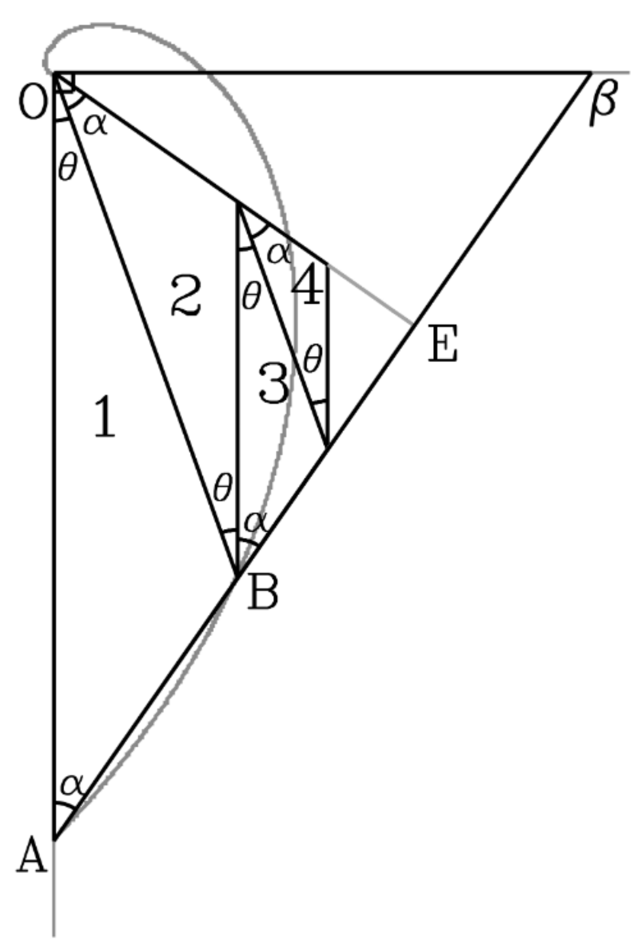

Figure 1.4 Rearrangement of the triangles in Figure 1.2. Length = AE + OE → OAsecα. Area = ΔOAE → $\frac{1}{4}OA^2\tan\alpha$, as θ → 0

$\frac{1}{4}OA^2\tan\alpha$. This demonstration has almost the simplicity of the *diknume* or 'look and see' proofs of Antiquity.

I said that Harriot also rectified the loxodromic spiral on the sphere. This is also fairly easy, provided it is known, as Harriot has shown elsewhere, that this part of the geometry of the sphere and its associated so-called nautical triangle involves simple proportion and not logarithmic as the other main part does. The result is then just $\frac{1}{2}\pi OA\sec\alpha$. He was less successful in getting the curved area swept out, plausibly estimating it as $\frac{1}{16}\pi^2 OA^2 \tan\alpha$, where the factor presumably arises as $\frac{1}{4}(\frac{1}{2}\pi)^2$, which would be ingenious. In truth, however, the factor should be $\ln 2$, and not $\pi^2/16$. There is not much difference numerically, so perhaps he had checked this by measurements on a globe. This was a problem eventually solved by Halley (1695) nearly 100 years later.

Harriot's rectification is, in its field, an isolated result. While having certain general elements, it does not indicate a fully formed general algorithmic approach, but takes advantage of specific special properties of the given curve. In this way, it falls naturally into the 'pre-calculus' period of the earlier seventeenth century, along with results by Roberval, Cavalieri, Fermat, Pascal and others. But it also differs from these by being a forerunner – *the* forerunner, in fact – and arising naturally as just one stage in an immensely structured solution of a practical problem. Had Harriot done *nothing* else, his rectifications and the conformality result or proof would preserve his name, and we should wonder what else he might have done.

One tantalizing further result, also connected with the subtle geometry of the sphere, deserves to be mentioned. In 1603 Harriot found the area of a spherical triangle in terms of the sum of its angles and the radius of that sphere. Having not completely avoided formulae so far, ignoring the principle of Stephen Hawking's publisher, namely that each equation reduces the audience by one half, I will give another:

Area of spherical triangle = $r^2(\Sigma - \pi)$

where π is the sum of the angles in a plane Euclidean triangle in radians and Σ is the sum of the angles of the spherical triangles, which is necessarily greater, as it happens. By 1627 Briggs and Hakewill recognized this as a 'new' result, that is, one not known or not known to be known to the Ancients. It was published by Girard in 1629 and Cavalieri in 1632.[20] Exactly why it interested Harriot is unclear, although he applied stereographic projection and conformality to it in a series of diagrams. The mirror image of this result appeared in the 1760s to J.H. Lambert (1728–77), the great polymath and Berlin academician, though it was not published until 1786.[21] Lambert was considering the notorious question of parallels or, as we would now say, hyperbolic non-Euclidean geometry (HNEG). In such a geometry the area of a triangle would be the negative of Harriot's result, namely $k^2(\pi - \Sigma)$, where k is some real number (and hence k^2 is positive). The meaning of this did not become clear until the 1820s with the work of Bolyai and Lobachevsky (and possibly Gauss), and even then there were still obscurities (what *was* k?), resolved later in the century by Beltrami in Italy, Poincaré in France, and Riemann and Klein in Germany.

This might seem a mere curiosity but for another parallel or correspondence that has remained little if at all remarked upon until now. All mathematicians

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20 A. Girard, *Invention nouvelle en l'algèbre* (Amsterdam, 1629); B. Cavalieri, *Directorium generale vranometricum, in quo trigonometricæ logarithmicæ fundamenta, ac regulæ demonstrantur, astronomicæ(que) supputationes ad solam ferè vulgarem additionem reducuntur* (Bologna, 1632).

21 J. Gray, 'Geometry', in T. Gowers (ed.), *The Princeton companion to mathematics* (Princeton, NJ, 2008), pp. 83–95 (pp. 87–8).

and many others have heard of non-Euclidean geometry, but even the former are usually unaware of the so-called fundamental formula (obtained by both Bolyai and Lobachevsky, though in quite different ways), which relates the angle of parallelism at a point to the distance to the parallel at that point:

$$E^{-d} = e^{-d/k} = \tan(\tfrac{1}{2}\alpha)$$

where k (or E) is an arbitrary constant. This has a rather modern look. The remarkable fact is that Harriot's calculation of meridional parts (his solution of the Mercator problem) by a method structurally equivalent to logarithmic tangents would be given by the identical formula, where d becomes the meridional part or extension of the meridian (or longitude) lines on a Mercator map and α becomes the colatitude; k becomes a constant that Harriot's method effectively determines for his specific problem (it is the scale factor between angles in radians and angles in minutes of arc). That there should be such a correspondence is probably not very surprising: the sphere is a surface of constant positive curvature, and the HNEG plane is representable as one of constant negative curvature, which (as Hilbert showed) can only be partially mapped by a Euclidean surface. I find it almost eerie that it was the same Harriot who had led the way with the appropriate triangular area. I am inclined to refer to this correspondence as the Harriot-Bolyai correspondence; I omit Lobachevsky's name only because I find Bolyai's paper the more interesting, despite its notation being more obscure. The relation between area and curvature was later much clarified by Gauss himself, the *sogenannte* Prince of Mathematicians, in his 'most elegant theorem', as he called it. In it, Gauss effectively generalized the area results of both Harriot and Lambert, showing that $\Sigma - \pi$, as above, was equal to the integral of the curvature over the surface. The angles were now formed by the geodesic lines on that surface.

I am not suggesting, of course, that Harriot had any insight into non-Euclidean geometry. I do not know whether he ever considered the question of parallels, an ancient and unsatisfactory topic before his time, which surfaced in England only later in the century (1663 in fact) when John Wallis gave a lecture at Wadham College, Oxford.[22] In this lecture, inspired by earlier Arab work, Wallis related the question to the existence of similar triangles (which in fact exist *only* in Euclidean geometry, otherwise triangles are only similar if they are congruent). This is all, even now, rather counter-intuitive: we absorb geometrical prejudices all too easily, and even mathematics students have to be told that, for example, parallelism and equidistance are not the same thing, coinciding only in the limiting case of Euclidean geometry. But these remarks do, I believe, show the continuity of mathematics and provide a further placing of Harriot in that great tradition.

Perhaps a rather lesser example might be added here. Harriot looked at another geometrical problem that had interested the Arabs, namely Alhazen's problem of

[22] J. Wallis, *Opera mathematica*, 3 vols (Oxford, 1693–99); J. Gray, *Ideas of space* (Oxford, 1989), p. 58.

determining the points of reflection of light travelling from A to B via a cylindrical or a spherical mirror. It turns out to be a biquadratic problem; in other words, that such an equation is its algebraic equivalent. Harriot solved it by a *neusis* method, that is, the fitting of a specific line into the diagram. Such methods go back to Antiquity where they were used by Archimedes, Apollonius and others. He also *seems* to have solved it algebraically; at least, a rather messy diagram shows the related cubic curve.[23] His solution was obtained again by Isaac Barrow (1630–77). In the meantime Christiaan Huygens (1629–95) had solved the problem by the intersection of a circle and a rectangular hyperbola, which turns out to be the *inverse* of the Harriot-Barrow curve, as one may call it.[24]

Inverse transformations, as it happens, also have the property of conformality, which I referred to above in connection with the Mercator problem and its solution by Harriot.[25] So again we have *tout se tient*, and the centrality of Harriot's ideas to those that recur in the subject is apparent. Even more striking is the fact that the method of inversion, best known to science through Lord Kelvin's electrical research, occurs earlier with Newton in the *Principia*, Book I, proposition LXXXII, where he relates the attraction at a point inside a sphere to that at an inverse point outside it, without, however, using such terminology. Oddly, this connection is not widely known.

Another area where Harriot forms part of a continuing chain is in binary arithmetic. Most sources locate the origin of this in an explicit sense in Leibniz, but again Harriot is the forerunner, with examples that could have been straight out of the New Maths of the 1960s.[26] He also uses the binary decomposition of integers to facilitate the calculation of exponentials or powers, but I suspect that that was an older tradition, perhaps going back to the Egyptian multiplication by duplication and addition. This makes Harriot an important link in the long tradition from at least Ah'med (Rhind Papyrus c. 1650 BC) to modern computer work. This work of Harriot includes examples of the so-called Gray binary coding. This is an arrangement whereby addition of a unit changes only *one* digit in the representation. In ordinary binary any number of the available digits may be altered, and this can be inconvenient. For example, 1110 + 1 = 1111 entails only one change, but 01111 + 1 = 10000, a total of five changes. A non-binary example

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[23] One has to be careful with such terminology, however. Few cubics were known at that time and probably not as such. Newton's full classification, according to which there were 78 distinct types, unlike the three main ones for quadratic curves, was not completed until the 1690s and was only published in 1704 as an appendix to the *Opticks*.

[24] It is of type 40 in Newton's classification.

[25] Actually, Harriot had two distinct solutions. In the earlier one he simply added secants. This is not unsatisfactory in practice, but it does involve increasing errors for high latitudes that are difficult to estimate.

[26] J.W. Shirley, 'Binary numeration before Leibniz', *American journal of physics*, 19 (1951), 452–4; A. Glaser, *History of binary and other nondecimal numeration* (Los Angeles, 1981), pp. 11–14.

would be 1999 + 1 = 2000, a total of four changes, but 2000 + 1 = 2001, just one change.

Harriot also worked extensively in applying quite sophisticated mathematics to the age-old secrets of shipbuilding and design. This was probably connected with specific problems current at the time (1608–10), involving Phineas Pett's *Royal Prince*, then being built at Chatham. Both Henry Briggs, already mentioned, and the master shipwright Matthew Baker, still active at the age of 79, were involved. The theory of conics was applied to interpolation for mast and spar dimensions, timber, and timber and space and similar. The 'lines' were also calculated by cubic and even quartic offsets, which might remind one of modern cubic splines. There is no hydrodynamic theory here, but a step forward nevertheless in what was necessarily a conservative trade. Harriot's detailed papers on this subject are rare survivals. Rather little was written down beyond confidential workshop manuals, and most of our modern knowledge is gained from recovered vessels such as the *Gustavus Vasa* or the *Mary Rose.* In this country, the old knowledge seems to have been lost in the course of the last generation. For example, shipbuilders in Brixham, such as Mr Blackmore or Upham's Yard, which built the *Mayflower II* in the 1950s, are no more. The same is true of the sail lofts, but in the USA men still build wooden boats, and elsewhere traditions survive, as I found when talking to some who were building a sizeable boat at Aswan a few years ago. Again Newton in the *Principia* included a bow-shape to reduce resistance in Book II, proposition XXXIV, scholium.

Many years ago, I wrongly denied Harriot's logarithmic tables the idea of a base, unlike those of Briggs.[27] While Napier's logarithms, which originate in kinematical ideas, implicitly have a base close to 1/e, their calculation did not involve that, whereas Briggs's base ten is closer and Harriot's should have been so described. Although not spelled out as such, Harriot's base is equal to $\exp(-2\pi / 360\times60)$, which he was able to approximate as 0.99970915409725778, accurate to the eleventh figure. He had to get this right to a very high level of accuracy because this number is eventually raised to very high powers (over 30,000), causing any serious error to become noticeable. He estimated this fundamental constant by finding *ce* in Figure 1.5 from $ae{:}ec = \sqrt{2}{:}1$; $a\hat{y}e$ is one minute of arc. He applied algebra to this, solved the resultant quadratic and, as we may verify (Harriot also made various checks), was correct to the second order, erring slightly at the third. This is the key place where he used the length of the spiral result; the $\sqrt{2}$ is the secant of 45 degrees or $\frac{1}{4}\pi$, the 45-degree rhumb line or loxodrome being the relevant one, as for it the meridional parts equal the difference of longitude involved. The spiral length used here acts as a proxy for the subsequent use of the integral of the secant function, which was unavailable to him.

[27] J.V. Pepper, 'Harriot's calculation of the meridional parts as logarithmic targets', *Archive for history of exact sciences*, 4 (1968), 359–413 (377).

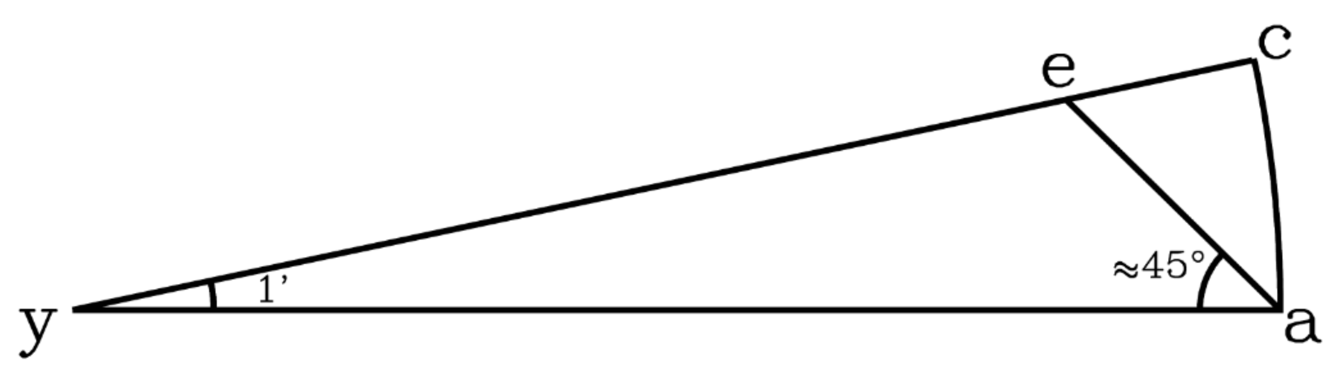

Figure 1.5 Calculation of the fundamental constant. *e* is chosen such that *ae:ec* = √2:1; then *yâe* ≈ 45°

Finally, comment is necessary on Harriot's work on ballistics.[28] Needless to say, Harriot had no difficulty in obtaining vertical parabolic orbits by combining uniform motion with orthogonal uniform acceleration, one of Galileo's best-known results (1638). However, he went further by considering resistance proportional to speed, thereby obtaining his tilted parabolic orbits. This topic was picked up again by James Gregory and Newton. As Lohne says, with publication, this topic would have been initiated by Harriot.

As Quinn and others have observed, Harriot was, in all his activities, essentially a problem-solver.[29] This is certainly true of his mathematics, in which the modern distinction between pure and applied was not made, and where he applied not only the existing mathematics of his time to various problems (although he did this too) but also created new ideas, notations, techniques and theories, as we have seen. He thus belongs to the great tradition of mathematicians from ancient times, Eudoxus and the Classics, through the Middle Ages, to Viète, Newton, Gauss, Maxwell, Poincaré and many others, including Sir James Lighthill in our own times.

This is no light claim. But in many ways, his work was before his time: he might well have made a mark with the same work 50 years or more later. He solved many specific and difficult problems. But if we have to look in general terms, what is most notable, apart from his devotion to both theory and practice, is his contribution to the move away from geometrical to algebraic formulations. This move is often regarded as an eighteenth-century development, but it was in fact a late sixteenth-century development, first by Viète and then by Harriot, and it has been the dominant movement of the four centuries since that time. Newton went the same way when young, even if later, under delusions about reconstructing an ancient knowledge that had not existed, he reverted to geometry, in public at

[28] J.A. Lohne, 'Essays on Thomas Harriot: II. Ballistic parabolas', *Archive for history of exact sciences*, 20 (1979), 230–64; Matthias Schemmel, 'England's forgotten Galileo: a view on Thomas Harriot's ballistic parabolas', in José Mortensinos and Carlos Solís (eds), *Largo campo di filosfare. Eurosymposium Galileo 2001* (La Orotava, 2001), pp. 269–80.

[29] Bennett, 'Instruments, mathematics, and natural knowledge', and D.B. Quinn, 'Thomas Harriot and the problem of America', in Fox (ed.), *Thomas Harriot*, pp. 9–27, esp. p. 9.

least.[30] Nowadays, some say that the algebraic victory was not total, with geometry and topology still fighting back. But that is another matter, with less effect on the three or four centuries after Harriot.

[30] It has been suggested that this explains much of the otherwise highly ambiguous workings of the *Principia.*

Chapter 2

Chymicorum in morem: Refraction, Matter Theory, and Secrecy in the Harriot–Kepler Correspondence

Robert Goulding

> This desire of glory, and to be counted Authors, prevails on all, even on many of the dark and reserv'd Chymists themselves, who are ever printing their greatest mysteries, though indeed they seem to do it with so much reluctancy and with a willingness to hide still, which makes their style to resemble the smoak, in which they deal.[1]

Introduction

'Doe you not here startle, to see every day some of your inventions taken from you[?]'.[2] So William Lower chided his friend Thomas Harriot in an often-cited letter written in February 1610 – one of the few letters which Harriot kept. The immediate occasion of his letter was Kepler's *New astronomy*, published the previous year, in which Europe's greatest astronomer had established, in his first two laws, the elliptical motion of the planets. 'I remember longe since you told me as much', recalled Lower, 'that the motions of the planets were not perfect circles.' So too, Harriot had been 'robd' of his work on specific weights of substances by Marino Ghetaldi and of his algebra by François Viète. Had Lower waited another month, he could have added an even more spectacular 'robbery': Galileo's *Sidereus nuncius*, and its descriptions of telescopic observations of the moon. Harriot had been making his own observations and drawings of the moon for several months before Galileo, through 'perspective trunks' of his own construction.[3]

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[1] T. Sprat, *History of the Royal Society* (London, 1667), p. 74.

[2] The text of this letter is partially printed in J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), pp. 400–401, from which all quotations here are taken.

[3] On Harriot's observations, see ibid., pp. 401–9; and J.J. Roche, 'Harriot, Galileo, and Jupiter's satellites', *Archives internationales d'histoire des sciences*, 32 (1982), 9–51, esp. pp. 9–12. For the important ways in which Galileo's understanding of his lunar observations outstripped Harriot's, see S.Y. Edgerton Jr., 'Galileo, Florentine "Disegno", and the "strange spottednesse" of the moon', *Art journal*, 44 (1984), 225–32; W.R. Shea, 'How Galileo's

Lower's letter was suffused with melancholy. His beloved son had just died (as he informs Harriot) and he reflected mournfully both on his loss and Harriot's constant intellectual misfortune. Yet he also offered himself and his friend consolation and remedy. For himself, he took strength in remembering Harriot's own example of submission to the will of God as he had borne his various misfortunes, so that 'onlie my wife with more griefe beares this affliction, yet now again she begins to be comforted'. But even if Harriot had accepted his intellectual losses with equanimity, Lower could not approve of his resignation, and offered him some blunt advice:

> Onlie let this remember you, that it is possible by too much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your Countrie and friends injoye the comforts they would have in the true and great honor you would purchase your selfe by publishing some of your choise works.

Lower's concerns were well founded. Despite being (by modern reckoning) the most accomplished early modern English scientist, Harriot published nothing of his mathematical or natural philosophical researches. To anyone who has spent time with his manuscripts, the reasons are not too hard to see. In the chaos of the thousands of pages which survive of his papers, one finds for the most part endless calculations, unlabelled diagrams, tables of observations and, *very* occasionally, an explanation or conclusion. Whether through excessive caution, insecurity or (as Lower suggested) simple procrastination, Harriot seemed unable to bring any project to a close, still less to prepare it for a wider public. Indeed, in his will, he appointed his friend Nathaniel Torporley not just to publish his works, but also to sift through his papers, distinguishing the 'cheife' writings from the 'waste papers', and then 'after hee doth understande them hee may make use [of them] in penninge such doctrine that belonges unto them for publique uses…'.[4] If Harriot *had* intended to publish his works during his lifetime, even the task of discovering which were of any significance was, it seems, beyond him.[5]

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mind guided his eye when he first looked at the moon through a telescope', in G. Simon and S. Débarbat (eds), *Optics and astronomy. Proceedings of the XXth international congress of history of science*, vol. 12 (Turnhout, 2001), pp. 93–109; S.Y. Edgerton Jr., *The mirror, the window, and the telescope. How Renaissance linear perspective changed our vision of the universe* (Ithaca, NY, 2009), pp. 151–67. On the possible influence of William Gilbert's pre-telescopic observations and map of the moon on Harriot's lunar drawings, see S. Pumfrey, 'Harriot's maps of the moon: new interpretations', *Notes and records of the Royal Society of London*, 63 (2009), 163–8.

[4] Shirley, *Thomas Harriot*, p. 2. See also R.C.H. Tanner, 'The study of Thomas Harriot's manuscripts: I. Harriot's will', *History of science*, 6 (1967), 1–16.

[5] For an example of the extraordinary labor required to turn Harriot's scattered notes back into a coherent narrative of scientific discovery and reasoning, see M. Schemmel,

All of this is quite well known. Indeed, Harriot's taciturnity has come to define him as much as his virtuosity. I recall these familiar facts in order to contrast the one time (so far as we know) that he made a concerted but unsuccessful effort to inform the wider world of some of his most important discoveries concerning the refraction of light. As I will argue, in his correspondence on this subject with Johannes Kepler, Harriot wished to reveal that he had some extraordinary knowledge in his possession; however, he was (as in all other things) unprepared or unwilling to reveal it all at once – particularly to Kepler, his only serious rival in the field of optics. In order to stake his claim to superiority to Kepler on the subject of refraction, without fully disclosing the grounds for it, Harriot adopted the persona and modes of communication of the alchemist. But, as I will go on to argue, Harriot's alchemical smokescreen was not just a convenient means of disguise; his study of refraction really was closely connected to his studies and experiments in alchemy.

Harriot's optics

In their posthumous survey of Harriot's manuscript remains, his executors found several large bundles of working papers devoted to optics.[6] Little attention was paid to these papers at the time, or even after their rediscovery in the late eighteenth century. It had long been known that Harriot had a special interest in refraction and that he had measured the refractive properties of many transparent materials,[7] but it was only in 1951 that John Shirley published a short note in which he suggested that Harriot was more than just a careful experimenter. After the death in 1640 of Walter Warner, Harriot's long-time collaborator, the mathematician John Pell made a record of conversations he had had with Warner on mathematical subjects.[8] Shirley reproduced the following conversation:

The English Galileo. Thomas Harriot's work on motion as an example of preclassical mechanics, 2 vols (London, 2008). Schemmel's thoughts on Harriot's failure to publish (pp. 21–3) are very well considered. On the differences in patronage between England and the Continent, and the obstacles to publication presented to someone like Harriot, see S. Pumfrey and F. Dawbarn, 'Science and patronage in England, 1570–1625: a preliminary study', *History of science*, 42 (2004), 137–88, esp. 162–4.

[6] For an account of the executors' survey of the manuscript remains, see R.C.H Tanner, 'Nathaniel Torporley and the Harriot manuscripts', *Annals of science*, 25 (1969), 339–49 (transcription of executors' description of the bundles of papers at pp. 346–9). An annotated version of the executors' description, in which items on the list are matched as far as is possible to surviving manuscript material, is found in J.A. Lohne, 'A survey of Harriot's scientific writings', *Archive for history of exact sciences*, 20 (1979), 265–311, esp. 275–85, and also 307 on the bundles of optical writings.

[7] Harriot revealed this quite openly in the correspondence with Kepler discussed below.

[8] Extant in BL MS Birch 4407.

> Mr Warner says that he had of Mr Hariot this proportion: As the sine of one angle of incidence to the sine of its refracted angle, by experience; so the sine of any angle of incidence upon the superficies to the sine of its refracted angle, to be found by supputation.[9]

In other words, given any two angles of incidence ϕ_1 and ϕ_2 at a refractive interface (see Figure 2.1), the refracted angles ρ_1, ρ_2 will be such that:

$$\frac{\sin \phi_1}{\sin \rho_1} = \frac{\sin \phi_2}{\sin \rho_2} = \kappa$$

where κ is a constant for the particular pair of media.

This was a spectacular discovery. The sine law of refraction (which is what this is) was supposedly discovered by Willebrord Snell around 1621, but was first published by Descartes in 1637. Here was apparent proof that Harriot (who died in 1621) had discovered the law first. The evidence, however, was questionable, coming as it did from a third-hand source writing 20 years after Harriot's death, when the Cartesian *Dioptrique* was already well known and based on the testimony of one of Harriot's most loyal supporters.[10] Shirley (who was little inclined even in his biography of Harriot to try to make sense himself of the mathematical papers) asserted that Harriot's own records of his refraction experiments had probably perished.[11]

Harriot's priority in the field of refraction was confirmed quite independently by the Danish scholar Johannes Lohne in 1959.[12] Lohne was researching the history of refraction and found in the University of Oslo Library a copiously annotated copy of Witelo, the thirteenth-century optical writer whose inaccurate

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[9] J.W. Shirley, 'An early experimental determination of Snell's law', *American journal of physics*, 19 (1951), 507–8 (507). The passage goes on to describe an experimental demonstration of this relationship which he (Warner) had performed. Shirley assumes this to be a description of *Harriot's* method. However, it seems to me to be identical to the method used in one of Warner's manuscripts (BL Add. MS 4395; refractive tables, experimental accounts and other relevant materials are scattered throughout the volume, with a tract devoted to refraction and the sine law at ff. 141–54). Here it is stated that the experiments were performed by Warner and Sir Thomas Aylesbury (another of Harriot's associates) *after* Harriot's death. See, for instance, f. 99r: 'Ex observationibus diligentissimis Tho. Aylesbury & Walt. Warner, Westmonasterii. Mense Julii 1627.'

[10] Pell himself observed that the proportion supposedly found by Harriot was the same as that related by Descartes.

[11] Yet another case in which the 'eight great folio volumes baffle the efforts of every succeeding student in the attempt to extract sense commensurate to Harriot's fame as a mathematician and to the sheer size of the collection'. R.C.H. Tanner, 'Thomas Harriot as mathematician: a legacy of hearsay', *Physis*, 9 (1967), 235–47, 257–92 (242).

[12] J.A. Lohne, 'Thomas Harriott (1560–1621): the Tycho Brahe of optics', *Centaurus*, 6 (1959), 113–21.

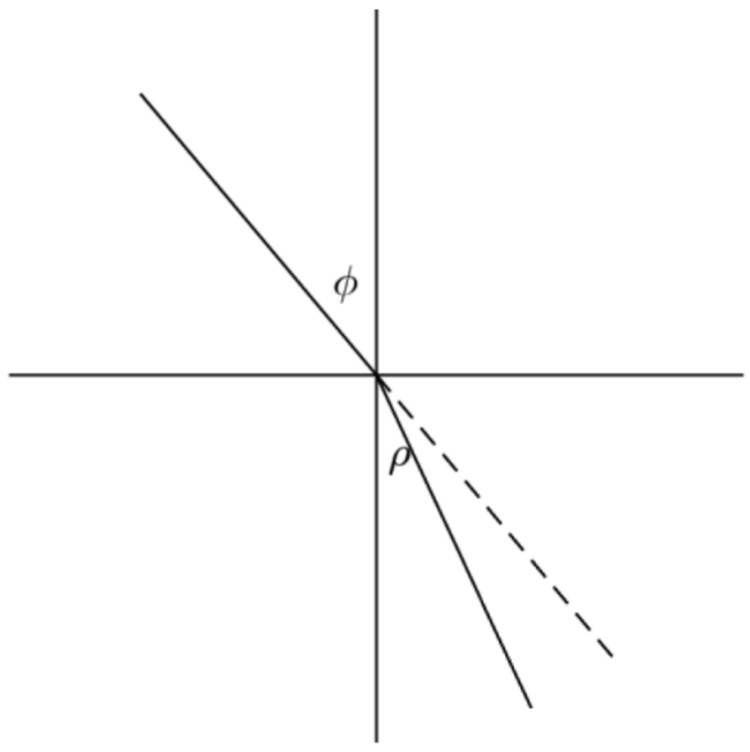

Figure 2.1 The refraction of light

table of refractions, derived from Ptolemy's *Optics*, remained authoritative into the seventeenth century. In the back of the volume was written a table of refractions. The writer summarized the results of Witelo and of Giambattista della Porta (who had published his *De refractione* in 1593), and included his own observations of refractions from air to glass and water. The observations were dated August 1597 and February 1598. Elsewhere in the book, next to Witelo's description of a peculiar optical phenomenon – seeing one's own image approach when walking in fog – the writer noted in the margin: 'T. H. Ego talem vidi, an. 158(?) august.29 in aedibus regiis otelands' ('I, T. H., saw such a thing in the royal estate at Otelands'). From this annotation, and a reference to 'Sion' alongside the refraction entries, Lohne, recalling that a Thomas Harriot had once worked for the Earl of Northumberland at Syon House, London, was able to identify the annotator.

Lohne then turned his attention to Harriot himself, examining the volume of the British Library manuscripts which was devoted to optics.[13] On the very first sheets he found tables of refractions for various media, for every degree between 0 and 90; they agreed precisely with the sine law of refraction and had obviously been calculated.[14] Elsewhere in the volume, he found tables of observed refractions dated 1602; they were far more accurate than the 1597 observations and each table now had an extra column for the refraction *per calculum* ('by calculation').[15] These calculated values were also derived from the sine law of refraction, which Harriot therefore must have discovered in the period 1597–1602 – that is, between his observations written in the back of his copy of Witelo (where he made no attempt

[13] BL Add. MS 6789 f. 88.

[14] That is, the ratio of the sines of each angle of incidence to the sine of the corresponding angle of refraction is constant throughout the table. In general, when I say that tables were compiled using the sine law, I mean that such a constant relationship between the sines of incidence and refraction angles can be discovered.

[15] BL Add. MS 6789 f. 266r: observations made in July 1602 of refraction from water to air, with calculated values (and the words 'ratio calculi' written in Harriot's cipher).

to calculate the values)[16] and this dated record of his calculations. Pell's third-hand account of Harriot's achievements in optics turned out to be quite accurate.

Lohne's subsequent articles drew attention to further refraction experiments recorded in Harriot's papers, his solution of specific problems such as the causes and size of the rainbow, and his engagement with a few other less well-known problems (such as Alhazen's reflection problem).[17] He said little about the context of Harriot's interest in optics, his initial investigations of refraction (in which he studied of Witelo and measured refractive angles) or his discovery of the law of refraction itself. Why did he conduct these experiments and mathematical explorations of refraction? In this chapter, I shall argue that Harriot's first investigations into refraction were connected with his abiding concern with the structure of matter, and that he continued to associate his refraction discoveries with his chemical and physical interests.

Harriot's surviving manuscript papers indicate that he put these studies aside shortly after the experiments of 1597 and resumed them around 1601 or 1602, now furnished with the sine law of refraction.[18] Elsewhere, I shall argue that Harriot's return to optics and discovery of a more accurate account of refraction were provoked by a request from his Welsh correspondent, John Bulkeley, concerning

[16] Further details on these experiments are found at BL Add. MS 6789, ff. 406–14. There Harriot records measurements of refraction from air to water, using a half-submerged staff; the experiments are dated August 1597 (and are the earliest dated refraction experiments found in Harriot's papers).

[17] Alhazen's reflection problem occurs in his *De aspectibus*, which was published alongside Witelo in 1572 and which Harriot also annotated in the Oslo copy. It is as follows: given a convex spherical mirror, find the point of reflection on the mirror such that an object at some *A* can be seen at some *B*. The problem cannot be solved by planar means; it requires a pair of conic sections or an equivalent construction. See J.A. Lohne, 'A survey of Harriot's scientific writings', 275–8 and 301–2; P.C. Fenton, 'An extremal problem in Harriot's mathematics', *Historia mathematica*, 16 (1989), 154–63 (which corrects Lohne's presentation of Harriot's solution in the previously cited article). Other important articles by Lohne on Harriot's optics include: 'Zur Geschichte des Brechungsgesetzes', *Sudhoffs Archiv*, 47 (1963), 152–72 (the product of Lohne's research on the history of the refraction law, which contains important observations on Harriot's manuscripts); 'Thomas Harriot als Mathematiker', *Centaurus*, 11 (1965), 19–45; and 'Kepler und Harriot: ihre Wege zum Brechungsgesetz', in F. Krafft, K. Meyer, and B. Sticker (eds), *Internationales Kepler-Symposium, Weil der Stadt, 1971* (Hildesheim, 1973), pp. 187–214. The latter can also be found as the sixth item in J.W. Shirley, *A source book for the study of Thomas Harriot* (New York, 1981).

[18] The earliest dated optical papers are from November 1596 (BL Add. MS 6789, f. 183v, containing some algebraic calculations related to his reading of Witelo and Alhazen). Dated papers both in the British Library manuscript and the Oslo Witelo continue to February 1598. The next paper in chronological order (BL Add. MS 6789, f. 266r) is the account of water-air refractions described above, which uses the sine law; there are several papers that extend this phase of Harriot's work to 1605 (and one paper, considered below, from 1613).

the heat that would be generated by a vast burning plano-convex lens, 48 feet in diameter. ‘Mr Bulkeley his Glasse’ surpassed, of course, the technological capacities of the Elizabethan age and was realized only in Bulkeley’s imagination. Nevertheless, it seems to have piqued Harriot’s interest. In the course of his careful calculations about the optical properties of the lens, Harriot first determined refraction values by interpolating from Witelo’s tables; he subsequently redid all the calculations using the sine law. This is the only place in Harriot’s papers where both interpolations from Witelo and calculated values are used side-by-side to solve a problem. It seems very likely that these sheets were written precisely at the time Harriot discovered the sine law of refraction between 1597 and 1602. Stimulated by a query from a friend that he wished to answer as accurately as possible, he stumbled upon the simple mathematical law, which (as the 1602 table shows) agreed far better with observed results than Witelo’s tables did.[19]

Harriot’s subsequent notes on refraction continued to focus on the burning properties of various glasses, to the neglect of more standard optical questions such as image-formation; such was no doubt the legacy of Bulkeley’s curious question. But throughout this period, he continued to measure the basic refractive properties of various media, and clearly still connected his optical investigations with his studies of the composition of matter. After 1602, with the sine law in hand, he carefully measured κ (the constant ratio of the sines of incidence and refraction) for a wide variety of refractive substances and was thus able to draw up calculated tables of refractions which were the most accurate to have ever been made. In addition, from at least April 1605, he had been making careful measurements of the colours which arose when white light passed through a triangular prism, and seems to have concluded (as Newton would some 60 years later) that colours corresponded to particular degrees of refraction of light.[20]

These were spectacular discoveries, certainly the most important new optical principles since Ptolemy, and Harriot, as a careful student of the older optics, could not but be aware of their significance. As usual, however, he made no efforts at first to communicate his optical discoveries beyond the walls of Syon House. But when Johannes Kepler published his revolutionary *Optics* in 1604, Harriot finally roused himself from his anonymity. He was surely aware that he himself had discovered something extraordinary about the behaviour of light besides which Kepler’s own account of refraction paled; at the same time, Kepler was very clearly the one person in Europe who *might* discover through his own efforts the sine law and all the other discoveries locked up within Harriot’s notebooks. Thus

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[19] The papers on Bulkeley’s glass are at BL Add. MS 6789, ff. 132–9. I am currently writing an extended analysis of the Bulkeley papers and many of the other optical notes, which will show more fully how Harriot’s interest in Bulkeley’s imaginary lens reignited (so to speak) his interest in optics and led to his discovery of the sine law.

[20] The earliest dated colour experiments are at BL Add. MS 6789, f. 190r, performed on 8–11 April 1605.

it was that Harriot undertook to publicize his discoveries, the only time he made such an effort for any of his 'choise works'.

Kepler and the problem of refraction

Late in 1604, Kepler published his *Optics*, a work often considered to mark the beginning of modern optics. Most famously, he explained the working of the eye as analogous to that of a *camera obscura*, correctly describing for the first time the upside-down image formed on the retina. Yet this was something of a digression on the way to his principal goal: an accurate description and precise measure of refraction – and, more particularly, of atmospheric refraction and its effect on astronomical observations.

This was no easy task: refraction had presented a problem to optical theorists since Antiquity, when it was first systematically investigated by Ptolemy. The Hellenistic astronomer, in his *Optics*, described a series of experiments to measure the angle of refraction between air, water and glass. He provided tables of refractions obtained by his experiments and gave a qualitative description of the phenomenon: when a ray of light (or actually of sight, as Ptolemy would put it) encountered a denser medium, it moved closer to the perpendicular in an effort to get through the resisting substance more easily. The more oblique the original ray, the more dramatically it swerved from its original route, seeking an ever more passable route, as it were. The phenomenon of refraction was clearly *regular*, but no-one (including Ptolemy) was able to come up with a simple mathematical description for it,[21] in marked contrast to the simple law of *reflection*, where the angle of the reflected ray always equalled that of the incident ray. Nevertheless, it was increasingly recognized during the sixteenth century that an understanding of refraction was essential for accurate astronomical observations.

Ptolemy's density explanation was followed by later optical writers, most importantly by Witelo, who not only echoed Ptolemy's physical description of refraction but also reproduced his tables in a slightly garbled form. Witelo, in turn, became the most important source of knowledge about optics for Renaissance natural scientists – including Harriot (as we have seen) and Kepler. The latter used Witelo's refraction table as a first approximation of he measure of refraction,

[21] Actually, Ptolemy's table (and later Witelo's) did exhibit mathematical regularity, in that the second differences of the refraction tables remained constant. Kepler himself noted this and took it as prima facie evidence that the refractions in Witelo's tables were not based on actual experimental results. See J. Kepler, *Optics. Paralipomena to Witelo and optical part of astronomy*, trans. W.H. Donahue (Santa Fe, 2000), pp. 128–9. For a full account of Ptolemy's refraction experiments, his 'fudging' of his data and his difficulty in formulating a 'law', see A. Mark Smith, 'Ptolemy's search for a law of refraction: a case-study in the classical methodology of "Saving the Appearances" and its limitations', *Archive for history of exact sciences*, 26 (1982), 221–40.

but not without criticism. In his 1604 work on optics (the first part of which he entitled *Paralipomena in vitellionem* or *Remarks on Witelo*) he rejected Ptolemy and Witelo's notion that the ray was seeking a route in compensation to its obstruction.[22] All the same, he was convinced that the measure of refraction was somehow connected with density or specific gravity and that a simple relationship must exist involving the density and the angles of incident and refracted light. His search for some such mathematical regularity in this phenomenon delayed the publication of his book by over a year. In May 1603 he wrote to a friend that 'measuring refractions, here I get stuck. Good God, what a hidden ratio! All the *Conics* of Apollonius had to be devoured first, a job which I have now nearly finished'.[23] His solution involved (as his own words suggest) the use of conic sections; specifically, he argued that the effect of refraction was analogous to reflection in a mirror shaped parabolically or hyperbolically.[24] The calculations were arduous and his conclusions unsatisfactory even to himself (as he admitted when writing to Harriot).

The beginning of the correspondence

In 1606, two years after the appearance of Kepler's *Optics*, Harriot initiated a correspondence with Kepler on the subject of optics and the theory of refraction.[25] In total, the two men exchanged five letters over three years, a correspondence which began with enthusiasm (at least on Kepler's part) but deteriorated into incomprehension and irritation. To introduce himself to his rival, Harriot chose to send news to Kepler via a merchant, Jan Eriksen, who travelled regularly between Prague and London, rather than write directly (thus beginning the correspondence with a certain evasiveness that would characterize all of his communications with Kepler). According to Kepler's own account (in his first letter to Harriot written in October 1606), Eriksen told Kepler of a man in England 'most skilled in all the secrets of nature' who had established new principles in optics which would 'show

[22] Kepler, *Optics*, p. 100. Kepler writes that Witelo's explanation of refraction treated light as if it were 'endowed with mind'.

[23] Letter quoted in introduction to Kepler, *Optics*, p. xii.

[24] Kepler's solution of refraction in terms of an imaginary reflection is remarkably (and, apparently, coincidentally) similar to the method used by Giambattista della Porta in his *De refractione optices parte* (Naples, 1593), a book that Kepler had looked for in vain (Kepler, *Optics*, p. 216). Harriot, however, did know della Porta's work, and in the Oslo annotations compared the refractions predicted by his reflection models against Witelo's tables and his own observations. See Lohne, 'Zur Geschichte des Brechungsgesetzes', pp. 157–9.

[25] This correspondence has been analysed elsewhere, most recently in A. Alexander, *Geometrical landscapes. The voyages of discovery and the transformation of mathematical practice* (Stanford, CA, 2002), pp. 115–24. See also Shirley, *Thomas Harriot*, pp. 385–8.

that my book on optics and all others ever written were not only incomplete, but even false'.[26] Faced with such ostentatious claims, it was to Kepler's credit (and entirely in accordance with his character) that he wrote Harriot a candid letter, admitting to the difficulties in his own optical theory and requesting Harriot's assistance in solving the remaining problems.

Reading between the lines of this first letter, it is possible for us to learn much more about what Eriksen had told Kepler of Harriot. First, Kepler was aware of the difficult personal situation in which Harriot found himself through his employer's suspected involvement in the Gunpowder Plot of November 1605, less than a year before. Although Northumberland had not, it seems, had any foreknowledge of the planned treason, still less any part in it, his cousin Thomas Percy had visited him on 4 November, apparently to gauge whether rumours of the next morning's events had reached the Earl, a Privy Councillor. Percy had dinner with Northumberland and his household (including Harriot) that evening. When the plot was uncovered and thwarted, Northumberland and those close to him fell under intense suspicion. The Earl would spend the rest of his life imprisoned in the Tower of London, and even Harriot was arrested and held for some time for questioning. Harriot was particularly suspected of having seditiously cast the king's horoscope; the articles of his interrogation, written out in James I's own hand, focus entirely on his supposed astrological services to the Earl. In his letter of defence, Harriot protested his complete detachment from politics and his love of the quiet, inoffensive pursuit of scholarship:

> I was never any busy medler in matters of state. I was never ambitious for preferments. But contented with a private life for the love of learning that I might study freely.[27]

Out of respect for the dangers of Harriot's predicament, Kepler invited him to reply in an unsealed letter 'if a sealed letter might invite suspicion'.[28]

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[26] J. Kepler, *Gesammelte Werke*, vol. 15 (*Briefe. 1604–1607*), pp. 348–52, letter 394 dated 2 October 1606 (this volume henceforth referred to as Kepler, *Briefe. 1604–1607*). The cited passage is on p. 348: 'esse in Britannia virum per omnia naturae arcana versatissimum … quique habet in Optica praecipue disciplina, principia nova, et vulgo ignota, ex quibus et meus Optices liber, et quicunque antea prodierunt, non tantum manci, sed etiam aberrantes, deprehenderentur'.

[27] This account of Northumberland's role in the conspiracy is based on G.R. Batho, 'Thomas Harriot and the Northumberland household', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 28–47, where the articles of interrogation and Harriot's defence are also reproduced.

[28] Kepler, *Briefe. 1604–1607*, p. 348: '… vel apertam ad me mittas Epistolam, si clausa suspicionem parere possit.' Kepler had even heard details of Harriot's interrogation and urged him to reject the superstitious practice of traditional astrology: 'Audio tibi malum ex Astrologia conflatum. Obsecro an tu putas dignam esse, cuius causa talia sint ferenda?' (p. 349).

Kepler also knew that Harriot's new results had something to do with refraction, since this was the main topic that Kepler addressed in his letter. Eriksen had also told him something of Harriot's colour experiments; Kepler reiterated his own theory from the *Optics*, which consisted of a variation of the standard perspectivist view that the colours arose from the admixture of darkness which the imperfectly transparent prism imparted to the light;[29] nevertheless, he desired a fuller, experimental elucidation from Harriot. Here, however, Kepler provided an unexpected detail which Eriksen must also have passed on to him. He wrote: 'I desire to know the origin of colors and their essential differences from you, *who have devoted effort to alchemy*' (emphasis added).[30] Kepler concluded his discussion of prismatic colours with another reference to Harriot's laboratory activities: 'If you are teaching *from alchemy* that all the colours inhere in the body of water, glass, crystal and so forth, then the way in which they are drawn forth and displayed on a piece of paper will be, it seems, thoroughly explained.'[31] In other words, Kepler had gleaned from his informant that Harriot was in possession of an account of the colours that arose from refraction, which attributed those colours somehow to the matter out of which the prism was made, and forged the link between the matter and the colours by means of alchemical knowledge about the matter itself. Previous studies of Harriot's optics, particularly the fundamental articles of Johannes Lohne, have encouraged us to see him as the 'Tycho Brahe of optics', a diligent observer and theoretician, concerned with the mathematical elucidation of the phenomena for their own sake – if not as a prototypical Newton.[32] It seems, however, that he had introduced himself to Kepler as an *alchemist* (thereby ironically having more in common with Newton than the flattering comparison was ever meant to suggest).

The degree to which Harriot himself associated optics and alchemy will be explored presently. But the fact that Kepler believed his correspondent was an alchemist provides a crucial insight both into the tone of Kepler's letter and into

[29] Witelo, *Optica*, 10.83. pp. 473–4 of F. Risner's edition (Basel, 1472). See also J. Gage, *Colour and meaning. Art, science and symbolism* (Berkeley, CA, 2000), pp. 122–6.

[30] Kepler, *Briefe. 1604–1607*, p. 348: 'Sic et colorum originem, et differentias essentiales abs te, qui Chymicis operam das, discere aveo.' The term 'chymicis' may perhaps be better rendered as 'chymistry' to denote the early modern form of alchemical practice and theory, following the lead of modern scholars of alchemy. See W.R. Newman, *Gehennical fire. The lives of George Starkey, an American alchemist in the scientific revolution* (Chicago, 1994), pp. 84–5; and W.R. Newman and L.M. Principe, 'Alchemy vs chemistry: the etymological origins of a historiographical mistake', *Early science and medicine*, 3 (1998), 32–65 (41–2).

[31] Kepler, *Briefe. 1604–1607*, pp. 348–9: 'Si docueris ex Chymia omnes colores inesse in corpore aquae, vitri, crystalli etc. de caetero ratio eliciendi et in papyrum exprimendi bene iam explicata videbitur.'

[32] Lohne, 'Thomas Harriot (1560–1621)', 119: 'As was to be expected Harriott also investigated the passage of sun rays in prisms – even more thoroughly than Newton did 60 years later.'

the subsequent development of the correspondence. Alchemists were, of course, notoriously secretive: advertising their extraordinary accomplishments while veiling their researches in deliberately obscure jargon and at times even intentionally misleading their readers. Kepler, on the other hand, very deliberately drew attention to his own openness and frankness, admitting to his errors and sharing his latest, unpublished thoughts on the phenomena of light. Moreover, he constantly invited Harriot to emulate his own example of candor. He published his *Remarks on Witelo*, he told Harriot, even though he was aware that the argument was not perfect. He revealed at length just what he had and had not tested by experiment, and what continued to puzzle him: 'You've read my book', he wrote, 'you know what I would like to learn from you.' He admitted that the complicated mathematical pattern that he thought he had spotted in refraction was probably wrong (as indeed it was), and exhorted Harriot to share with him 'freely and frankly' ('liberaliter et candide') both his experimental results and the cause of the angles being the size they were: 'I await from you a complete description of the media and instruments you have used.' After presenting and criticizing his own inconclusive thoughts on the rainbow, Kepler appealed again to Harriot: 'give me the measures of all refractions in your experiments, then everything else will be easy'.[33]

If Kepler saw himself as a model of openness, he was already concerned about the tone with which Harriot had introduced himself. His extravagant praise of Harriot at the beginning of his letter is at least semi-ironic: a man 'skilled in all the secrets of nature'. Surely the bold claims of this alchemist must have seemed all too similar to those of the innumerable purveyors of 'secrets' who plyed their wares around Europe.[34] Kepler hinted openly at his estimation of Harriot in his final exhortation to the English scientist: 'Now you, O excellent initiate of nature, reveal the causes.'[35] This very same phrase had also appeared in Kepler's *Optics*, where it was used as a rather humorous acclamation to Giambattista della Porta, the master of secrets whose *Magia naturalis* obscurely and evasively suggested a prototype of Kepler's theory of the eye: 'Indeed, thou hast blessed us, O excellent initiate of nature!'[36] It was now up to Harriot to prove that he was something more than a common hawker of mysteries.

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[33] Kepler, *Briefe. 1604–1607*, p. 348: 'Lecto meo libro, ignorare non potes, quibus in quaestionibus a te cupiam erudiri'; '… me naturam lucis penitus ignorare'; 'At quae sit colorum forma, quae differentia specifica, plane ignoro' (p. 349); 'Tu vero beaveris me, ubi mihi mensuras has in variis liquidorum generibus communicaveris liberaliter et candide …'; 'Itaque circumstantias omnes et mediorum et instrumentorum abs te expecto' (p. 350): '… ostende refractionum omnium mensuras in experimentis tuis, tunc caetera omnia erunt expedita.'

[34] On the contemporary mania for 'secrets', see W. Eamon, *Science and the secrets of nature* (Princeton, NJ, 1996).

[35] Ibid., p. 352: 'Tu igitur O Excellens naturae mysta dic causas.'

[36] Kepler, *Optics*, p. 224 (on p. 209 of the Frankfurt, 1604 edition of Kepler's *Paralipomena*): 'Equidem beati nos, O excellens naturae mysta …'

Revelation and concealment: Harriot's first reply

Harriot had taken a great risk in initiating a correspondence with Kepler. The German astronomer was the most highly regarded optical theorist in Europe; if Harriot were to share his optical discoveries with anyone, Kepler was surely the one person most qualified to appreciate them. On the other hand, he must have been aware that his research was in no state to be published. Even though he seemed to hint in his reply that a book on the optics of the rainbow was forthcoming, there is no sign anywhere in his optical papers of even the beginnings of such a project.[37] Perhaps he concluded that if he were to reveal his results 'freely and frankly', as Kepler exhorted him, he would surely see yet another one of his 'choise works' claimed as the invention of another. Kepler's insistence on having the actual experimental data ('then everything else will be easy') could not have reassured him very much. I believe that Kepler's acceptance of Harriot's self-portrayal as an alchemist provided him with a solution: in his reply to Kepler, he played the role of a mysterious 'initiate of nature' to perfection, revealing his discoveries only to shroud them in deeper obscurity.

Harriot wrote his letter two months after Kepler had written his. It seems that Eriksen had delivered Kepler's letter to him and was awaiting an immediate reply to take back to Prague. Harriot complained that there was no time to reply in full to all of Kepler's queries; moreover, he claimed, he was so ill that 'it is difficult for me to write or think or argue clearly about anything at the moment'.[38] Notwithstanding his incapacity, Harriot's evasive and highly argumentative reply fills four folio pages of Kepler's collected works. He began by fulfilling Kepler's repeated request for experimental data, but in such a way that very little real information could be extracted from it. He listed 15 transparent substances, giving their specific gravities and the angle of refraction from the substance to the air for a single angle of incidence (30°).

Taking into consideration the kind of information that Kepler wanted about refraction, Harriot's table was unsatisfactory in several ways. First, in the tables in his *Optics* Witelo always recorded the refraction *from* air *to* the transparent substance (water or glass); since Harriot provided his data for the reverse situation (from transparent substance to air), there was no way to compare his results with Witelo's. Moreover, by giving only a single example for each substance at a single angle of incidence, he made it impossible to discern whether there might be any mathematical pattern in the refractions. To make any guess at the mathematical

[37] Harriot's reply to Kepler is at Kepler, *Briefe. 1604–1607*, pp. 365–8, letter 403 dated 2 December 1606. Near the end of the letter (p. 368), he writes: 'Sed quando scripsero de Iride videbis … rationes et causas …'

[38] Ibid., p. 365: 'Ad singula respondere una vice nimium est, praesertim hoc brevi tempore quo responsum meum expectatur; sed maxime ob malam valetudinem qua nunc ita affectus sum, ut mihi molestum est [sic] vel scribere vel accurate de aliqua re cogitare et argumentari.'

relationship, one would need very accurate results for several angles of incidence – and the same angles for different substances. Harriot certainly had such tables, both carefully measured by direct experiment and calculated using the sine law; he had even compared his tables entry by entry to Witelo's tables and had investigated the size of the error introduced by the use of Witelo's values instead of the precise, calculated refractions in his studies on Bulkeley's glass. As such, Harriot had at his fingertips precisely the kind of information that Kepler had requested and expected from him. He failed to provide it and presented instead a table of very limited utility. The only thing which can be inferred from the table is that (*contra* Ptolemy, Witelo and indeed Kepler), the degree of refraction does not correlate with the density of the substance. There is no denying that this is an important result; however, it revealed to Kepler only that his own density theory of refraction was incorrect, without giving him the opportunity of correcting it, still less of recovering Harriot's mathematical law.

Harriot's curious choice of information to share with Kepler was quite deliberate: it advertised his possession of optical knowledge (and demonstrated his superiority to Kepler as an optical experimenter), yet crucially withheld the *meaning* that Harriot had derived from this data. Such an interpretation of Harriot's motives is confirmed by the teasing, taunting way in which he described the table in his letter:

> This table tells you more than could be said in your whole, long letter to me. I leave it to you to make sense of it … I have disclosed these things to you [*haec intimavi*] so that you can either share in my conclusion concerning certain optical matters, or it can be an incentive for you to make further observations of your own.[39]

Harriot was practising a strategy of 'concealment through revelation': making knowledge available – even, as the verb *intimavi* suggests, making *secret* knowledge available – but only in such a way that it still remained his exclusive possession. Such an evasive style of scientific communication resembles the 'principle of dispersion' used by contemporary alchemists. As Lawrence Principe has shown, this method, first employed by the ninth-century Arabic alchemist who went by the pseudonym of Jabīr, was enthusiastically adopted by early modern European practitioners of the art.[40] Principe argues that the most secretive alchemical authors

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[39] Ibid., p. 366: 'Alloquitur te ista tabella plura quam explicari possunt magnitudine tuarum litterarum. Quae linquo tuo ingenio ad speculandum … Haec intimavi, ut vel mutes sententiam de aliquibus opticis, vel ut ansam tibi praebeant ulterius observandi.' In the passage omitted by the ellipsis, Harriot spells out the lack of correlation between density and refraction.

[40] L.M. Principe, 'Robert Boyle's alchemical secrecy: codes, ciphers and concealments', *Ambix*, 39 (1992), 63–74, esp. 68, where (in a manner very similar to Harriot's) Boyle gives deliberately vague instructions about a substance synthesized in a

often did in fact reveal the entirety of their discoveries, but only by scattering the details throughout several separately circulated writings. A single work might present an apparently complete account of the alchemical process, but could not be interpreted (and was, in fact, quite worthless or actually misleading) unless the diligent reader had collected *all* of their writings; only then would he be able to put together the puzzle. In just the same way, Harriot made a show of candour by sending Kepler a table that was all but meaningless to anyone who did not have access to Harriot's refractive experiments *in toto*.

Harriot's reflection theory of refraction

Rather than answer any of Kepler's other questions about his optical researches, Harriot took the opportunity (despite the alleged pressures of time) to attack Kepler's description of refraction in his recently published *Optics*. He framed his critique as an imagined dialogue between himself and Kepler, whom he made to play the role of an Aristotelian critic. The phenomenon of refraction, he wrote, had nothing to do with the impeding of light, as Kepler had claimed in his *Optics*. It was, rather, a species of *reflection*. To support this notion, he began from the phenomenon of partial reflection from a refractive interface. When one shines a beam of light onto such a surface, most of the light is refracted and follows an oblique path through the medium; some of the beam, however, is reflected off the surface. It must be the case, he argued, that different parts of the refractive surface provided a passage for light, while other, immediately contiguous parts obstructed it completely. The surface was discontinuous, a fact that pointed to the conclusion (Harriot insisted) that all matter was made up of atoms and void. Light reflected from the surface was obstructed completely by an atom; light that followed an oblique path into the material was also reflected, but *between* the atoms that made up the transparent matter:

> And so, a solid transparent body, which seems to the senses to be continuous in all its parts, is in fact really not continuous. Rather, it has corporeal parts which resist rays, and incorporeal parts which admit rays. It is in just this way that refraction is nothing other than internal reflection; and even if the part of the ray that is admitted within the material seems to the senses to be straight, it is in fact really composed out of many [straight lines]. But let us make a stop here.[41]

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way 'I may elsewhere teach you' out of an 'obvious Vegetable'. See also W.R. Newman and L.M. Principe, *Alchemy tried in the fire. Starkey, Boyle, and the fate of Helmontian chymistry* (Chicago, 2002), pp. 186–7.

41 Harriot's argument on partial reflection and refraction as a species of reflection occurs in Kepler, *Briefe. 1604–1607*, pp. 367–8, at the end of which is the cited passage: 'Ergo corpus densum diaphanum quod sensu videtur esse continuum per omnes partes, revera continuum non est. Sed habet partes corporeas quae radiis resistunt, ry partes

From this passage in his letter to Kepler, it seems that Harriot imagined refraction and reflection to take place as in the diagram below (Figure 2.2). In this hypothetical reconstruction (which will be justified below), matter is conceived to be a regular arrangement of atomic particles and intervening void. Its interaction with light is very much like that of an array of pegs into which a marble is dropped from above. When the marble is released, on occasion it will hit the first peg and bounce away; more often, however, it will fall through the machine, bouncing from peg to peg and eventually leaving the array at a very different path. The oblique but regular zigzag path of the admitted ray – which, as Harriot wrote, appears straight to the naked eye but is actually made up of *many* straight lines – is what is taken to be the bent, refracted ray.

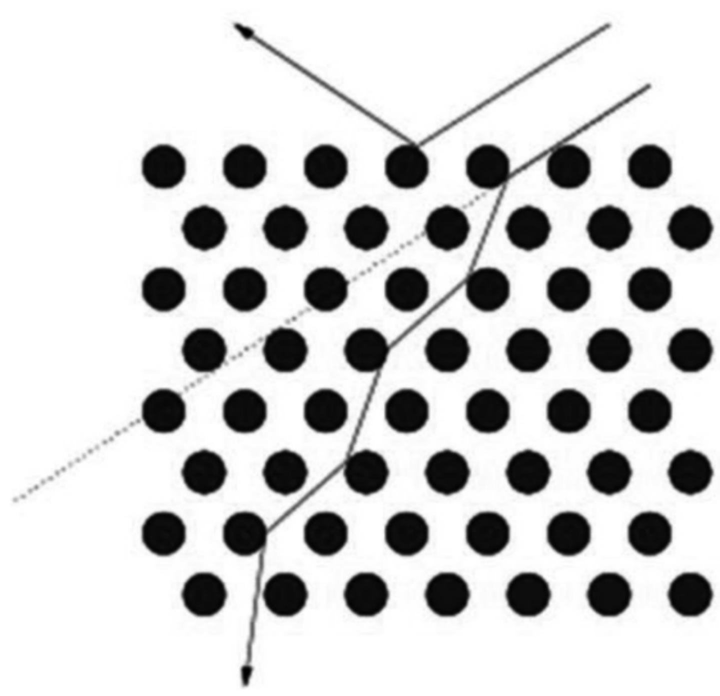

Figure 2.2 Passage of refracted light through a solid medium

Harriot did not explicitly tell Kepler that the transparent substance is such a regular array of atoms, nor did he really explain what he meant by claiming that 'refraction is nothing other than internal reflection'. But I believe that this is indeed the model that Harriot had in mind, and my reconstruction of his explanation of refraction receives support from his own hand. In his manuscript writings on optics, he sketched several diagrams which support this view. For instance, on one sheet he drew zigzag reflections from four evenly positioned spheres. Next to this diagram, he reproduced the same zigzag path, bounded within a pair of parallel lines, and another line which shows the direction the ray would have taken if it had not collided with the first sphere.[42] The diagram is reproduced below in Figure 2.3. There can be little doubt that the diagram shows refraction as multiple reflection, the parallel lines bounding the zigzag ray representing the *perceived* solid, refracted ray deviating from the unrefracted, direct ray.[43]

incorporeas radiis pervias. Ita ut refractio nihil aliud est quam interna reflexio, et pars radii intro recepta etsi videtur sensui esse recta, est tamen revera composita ex multis. Hic sisto.'

42 BL Add. MS 6789, f. 336r. The first part of this diagram is reproduced in Alexander, *Geometrical landscapes*, p. 121.

43 A very carefully drafted illustration of refraction as reflection is found at BL Add. MS 6789 fol. 210r, where Harriot drew the crooked refracted rays for incident rays at 30° and 60°.

There is one manuscript page, however, which conclusively links Harriot's zigzag diagrams with his letter to Kepler on refraction. On 7 October 1606, Harriot observed a 'rainbow in the shape of a hyperbola', of which he made a rough sketch.[44] It should be noted that the date of this observation was less than two months before he wrote his reply to Kepler. Above the drawing of the rainbow appear some of his characteristic zigzag refractive paths. On the reverse of the sheet bearing these diagrams is a table of refractions which was the original of the table that he sent Kepler in his letter![45] Little more is needed to confirm that, at the time he wrote to Kepler, he had a 'zigzag through a pinboard' model of refraction in mind.

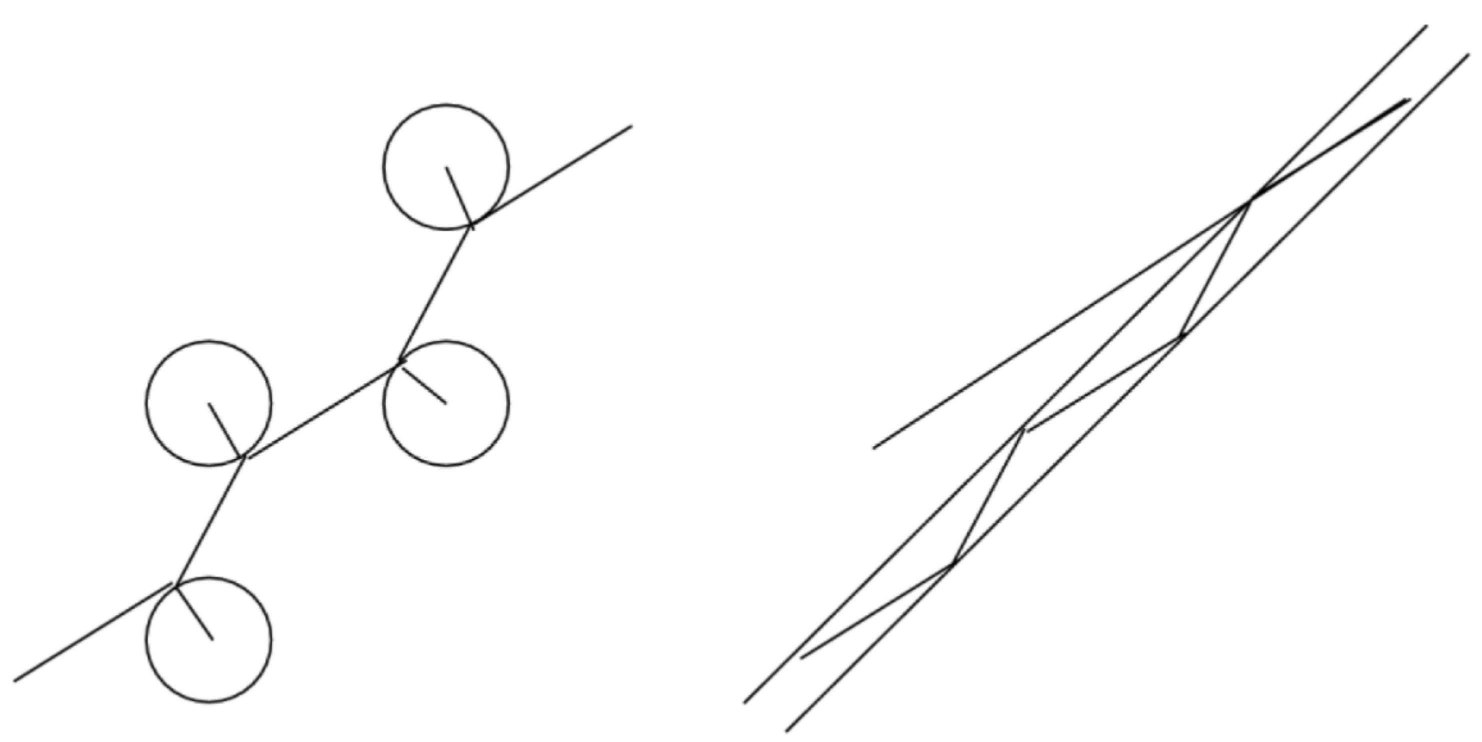

Figure 2.3 Diagram of refraction as reflection (after BL Add. MS 6789, f. 336 recto)

As I have reconstructed it, Harriot's theory of refraction was tied up with a particular atomic theory of matter. Another source (one of the most important documents on Harriot's atomism, although written from a critical point of view) confirms that atomism indeed played a central role in Harriot's optics. This is a single sheet of paper written by Nathaniel Torporley, headed 'Synopsis of the controversie of atoms'.[46] Torporley, although one of Harriot's closest friends and later to be his literary executor, was considerably more conservative than Harriot in his religious and philosophical beliefs, and listed objections to an atomistic theory of matter, using a colourful metaphor of battalions and skirmishes. He began by stating the two possible views: 'Either Atoms are, or bodies are compounded and

[44] BL Add. MS 6789 f. 331r.

[45] BL Add. MS 6789 f. 331v. The same substances are listed as in the letter, in almost the same order, with precisely the same numerical values both for the specific weights of the substances and the refractions of the 30° ray.

[46] BL MS Birch 4458, f. 6 (in Torporley's hand) and ff. 7–8 (fair scribal copy). Edited in J. Jacquot, 'Thomas Harriot's reputation for impiety', *Notes and records of the Royal Society*, 9 (1982), 164–87 (183–6). Jacquot also notes the similarity between the atomic theory attributed to Harriot here and that which appears in his correpondence with Kepler.

resolvable into nihilum.' Torporley went on to clarify his own position and reveal the object of his criticism:

> Wee disprove demonstratively the first, and so conclude the second, where opposing chiefly T. H. wee first lay downe his maximes with some following them in number 12.

'T. H.' is, of course, Harriot; it has been plausibly argued that Torporley wrote this 'Synopsis' in response to the debates going on among Harriot and his friends, in order to focus the discussion.[47] Torporley assembled three 'squadrons' of arguments against Harriot. The second and third relate to physics and statics, and each contains a single argument. The first squadron is optical and contains no fewer than seven arguments concerning refraction through a crystal ball, the first of which recalls Harriot's explanation of refraction in his letter to Kepler:

> it followeth that refraction differeth not from reflexion but in many repetitions thereof. Then in a cristall ball, the site varied, the radiation would differ for the different position of the superficies of Atoms.

Torporley had in mind a model of refraction precisely as we have described it above, and singled out the obvious flaw in such a refractive theory. Let it be granted that the refracted ray enters the transparent medium as Harriot claimed and that the apparently single, oblique ray is in fact a pencil bounding a regular, zigzag passage from atom to atom. Then the angle of refraction will be entirely depend upon the part of the ball-like atom on which the light is first incident, and *not* solely on the angle of incidence (as it must be to save the observed phenomenon of refraction). If the light only narrowly grazes the first atom it meets on the transparent surface, one would expect it hardly to deviateat all. But if light were to fall at the same angle, though slightly displaced so that it hits the first atom in a different place, its refractive angle would be quite different (see Figure 2.4). Other, similar arguments follow, which apply to the same theory of refraction: for instance, that the actual angle at which a ray of light leaves the crystal ball will be finally determined not by the whole refractive medium but by its impact with the very last atom it meets

[47] Jacquot, 'Harriot's reputation for impiety', p. 186 argues that this is the attack on Harriot's atomism that Torporley referred to in his *Corrector analyticus* (a manuscript tract written after Harriot died, attacking errors in the printed *Praxis* on algebra). John Henry, on the other hand (J. Henry, 'Thomas Harriot and atomism: a reappraisal', *History of science*, 20 (1982), 267–96 [286–7]) notes that the pompous title Torporley uses in the *Corrector* – *Refutatio philopseudosophiae atomisticae* – could not refer to this sheet, since it is of an informal nature, written in English and uses a whimsical framing metaphor. The extremely compressed description of Harriot's atomistic beliefs in the extant manuscript sheet also suggests that Harriot was still alive and arguing these points; Torporley, in other words, was only noting them down as a contribution to an ongoing debate.

before leaving the ball; one should thus be able to change the measured refractive properties of the ball by shifting a single atom.

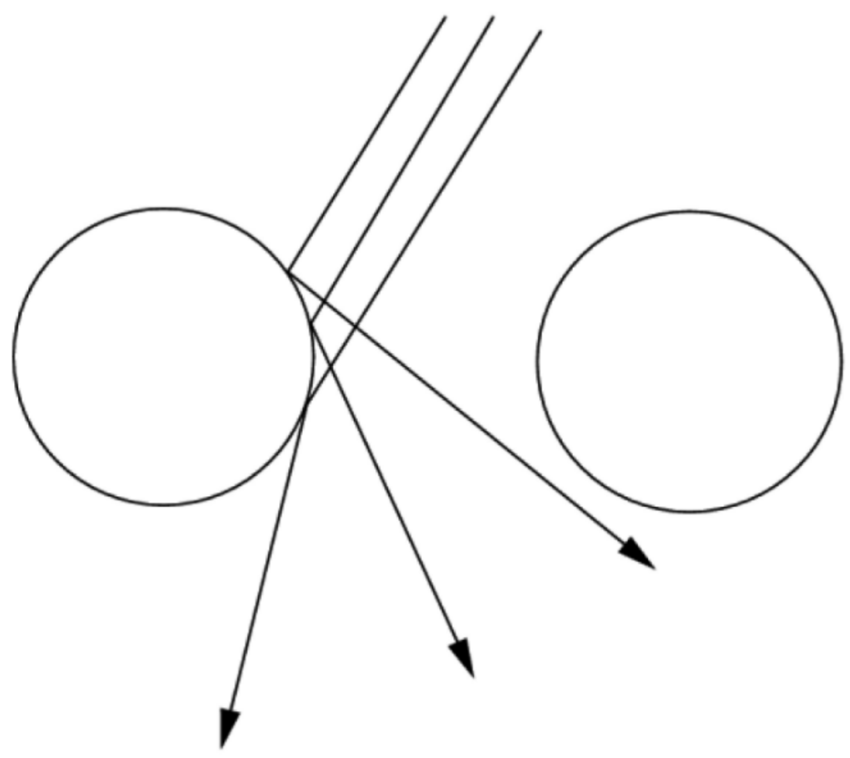

Figure 2.4 Torporley's objection to Harriot's refractive theory

The date of Torporley's criticisms is unknown; nor is there any sign in Harriot's manuscripts that he took account of them by modifying his theory.[48] In his letter to Kepler, he certainly admitted no doubts about the theory, but nor, on the other hand, did he make his theory explicit to Kepler. Whether because he was aware of problems in the theory or because this was one of his discoveries in optics that he wished to do no more than hint to his correpondent about, Harriot told Kepler only that the simultaneous reflection and refraction observed at the same point demonstrated the discontinuity of matter and that refraction was a kind of internal reflection.

In any case, apart from this brief episode of relative lucidity, Harriot cultivated throughout the letter an air of alchemical mystery. He would say nothing, he wrote, about the rainbow 'because of the mysteries which it holds', and even after his brief, one-sentence description of the internal reflections and refractions which were responsible for this phenomenon, he added, 'but I have said nothing, because of the mysteries which lie hidden there'.[49] He saved his mystagogic tour de force for the end of his letter. Although (he admitted) he had concealed much, by revealing his atomic theory of refraction:

> I have now led you to the doors of the house of Nature, where her secrets lie hidden. If the doors are too narrow for you to enter, then mathematically abstract

[48] However, as one can see in the doodles on f. 336r to the right of the diagrams of refraction as repeated reflections, Harriot may have been thinking about Torporley's objection regarding the sites of reflection on the surface of an atom.

[49] Kepler, *Briefe. 1604–1607*, p. 368: 'Quod ad colores attinet in illorum ratione magna sunt mysteria non illico expandenda'; 'Nihil tamen dixi ob mysteria quae latent'.

> and contract yourself to an atom and then you will easily enter. Later, when you leave, tell me what marvels you have seen.[50]

Alexander has connected this very passage to contemporary English exploration narratives and their mythologizing of the continental interior, demonstrating this in part from maps of Virginia prepared under Harriot's direction.[51] Without at all dismissing this interpretation, another is possible, quite in keeping with the role of the elusive alchemist that Harriot adopted in his correspondence with Kepler. His atomistic fantasy is, I argue, also a close adaptation of a famous passage from the *Corpus hermeticum*, treatise XI (which also apparently inspired a fellow atomist, Giordano Bruno).[52] In this long encomium to the soul, the author praised its ability to travel anywhere, from the fire of the sun to the edges of the cosmos, where it might gaze directly upon what lies beyond. One notes here an emphasis upon a direct visual experience of reality, which Harriot also urged upon Kepler: he would need to *see* the truth to understand it. But the author of the *Corpus hermeticum* went yet further: having ranged through the universe, he promised, the soul would gaze directly upon God and understand him. How could it gain such a privileged vision? The ancient author wrote:

> Make yourself grow to immeasurable immensity, outleap all body, outstrip all time, become eternity and you will understand god.[53]

The parallel here is quite striking. Just as the hermeticist had to *expand* himself to understand God, so too, according to Harriot, would Kepler have to *contract* himself to understand matter. Harriot seemed to be taking his role as a *mysta naturae* very seriously.

'In the manner of the alchemists': Kepler's reply

Kepler's reply to Harriot's letter came eight months later.[54] The hermetic resonances of Harriot's language, his evasiveness and mystification had not at all escaped him. It is understandable that Kepler showed some irritation at Harriot's letter; his expectations of alchemical obscurantism had, it seemed, been confirmed. While

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[50] Ibid.: 'Iam duxi te ad fores domiciliorum naturae ubi latent eius arcana. Si non possis intrare propter illarum angustias, tum mathematice abstrahe et contrahe teipsum in atomum et intrabis facile. Et postquam egressus es dic mihi quae mirabilia vidisti [sic].'

[51] Alexander, *Geometrical landscapes*, pp. 123–4.

[52] F. Yates, *Giordano Bruno and the Hermetic tradition* (Chicago, 1964), pp. 198–9.

[53] B.P. Copenhaver, *Hermetica. The Greek Corpus hermeticum and the Latin Asclepius in a new English translation* (Cambridge, 1992), p. 41.

[54] J. Kepler, *Gesammelte Werke* vol. 16 (*Briefe. 1607–1611*), pp. 31–2, letter 439 dated 2 August 1607 (this volume henceforth referred to as Kepler, *Briefe. 1607–1611*).

he praised Harriot's discoveries, he did so in much the same tone in which they had been presented to him. Harriot's reply was a great 'treasure chest' (*thesaurus*) and Kepler 'marvelled' at the table and the lack of correlation between density and refraction. 'Who would have believed that the spirits of wine were more refractive than spring water?', he mused.[55] But he also noted that Harriot had excluded all the information he had requested and had made his results impossible to use for gaining a deeper understanding of refraction. Harriot had offered no explanation for the unexpected differences in refraction, content merely to negate Kepler: if the degree of refraction was not connected to density, was oiliness or viscosity, or some other material property responsible for the optical variation in transparent materials? And he also recognized that Harriot had constructed his table so as to exclude the possibility of comparing his measurements with the refractions that Witelo had recorded: 'I should have preferred it if all the rays had been incident from the air, to the dense substance, at an angle of 30°.'[56]

Kepler was also clearly annoyed that Harriot had made him impersonate an Aristotelian, as if his optical theories (while by his own admission imperfect) were nothing more than a rehashing of scholastic natural philosophy.[57] Yet it was his correspondent's Hermetic obfuscation which provoked his sharpest rebuke:

> Now, you bring the whole subject to a dilemma, and (in part by argument, and in part by allegory, just like the alchemists [*chymicorum in morem*]), you mockingly seem to send me off to the atoms and natural vacua. But, even if it seems absurd to *you* that the same point, at the same instant, should both transmit and reflect a ray, it is not absurd to me.[58]

Indeed, as Kepler went on to say, he had been very careful in his *Optics* to construct a notion of 'pellucid', which shared in both transparency and opacity and hence, *qua* transparent, admitted a ray of light while at the same time, *qua* opaque, it

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55 Ibid., p. 31: 'Quis credidisset spiritus vini maiorem esse refractionem, quam fontanae?'

56 Ibid.: 'Deinde maluissem ex aere incidissent omnes radii in denso angulo 30.' Kepler reiterated this point in his final letter to Harriot. See ibid., pp. 250–51, letter 536 dated 1 September 1609, p. 250: 'I wanted to compare Witelo's water observations with yours, but could not because the two of you use different angles of incidence.' ('Nam erat mihi in votis comparare observatum Vitellionis circa aquam cum tuo; id non potui, quia vobis sunt diversae incidentiae'.) This statement is followed by extracts from their tables, illustrating the difficulty.

57 Ibid., p. 32: '… fingis me respondere … Ego vero et repudiavi considerationem finis seu boni in legibus opticis …' ('… you make me respond … But I too reject any consideration of the end or the good in optical laws').

58 Ibid.: 'Tu vero rem ad contradictionis bivium pertrahis, et partim argumentando partim allegoriis, chymicorum in morem, ludendo me ad Atomos et vacua Naturae ablegare videris. At quod tibi videtur absurdum, idem punctum eodem instanti et transmittere radium et repercutere, id mihi absurdum non est.'

reflected a portion of the same ray. Such an argument was unlikely to impress Harriot. In fact, in his reply to this letter, he told Kepler that he was moved to reread the beginning of the *Optics*, but was no more impressed than he had been at his first reading: 'I am amazed if the assumptions and arguments given there satisfy you; they do not satisfy me.' Transparency and emptiness had to go hand in hand, he continued to insist.[59]

Harriot's alchemical optics

Quite apart from their disagreement over the means of refraction, Kepler's riposte to Harriot that he was behaving 'just like the alchemists' confirms that he disagreed with Harriot not only on physical grounds, but also because of the manner that the Englishman had adopted. Harriot and Kepler's correspondence continued with one further exchange of letters, in which Harriot continued to be cryptic and evasive, presenting himself as an alchemical adept, and Kepler's criticisms became more pointed. Harriot, for instance, dismissed Kepler's own optical experiments with turpentine, which did not accord with his. Harriot revealed that his 'turpentine' was in fact a very light oil distilled from the original oil and that 'these two oils are very well known to any skilled alchemist or distiller'.[60] Kepler replied, rather lamely, that he was not asking questions about turpentine as an alchemist, but as an (experimental) observer.[61]

The impression one gets from reading Harriot's side of the correspondence is that he gratefully accepted the role of mystagogue and alchemist with which he had been introduced to Kepler, and which Kepler in turn had offered to him in his first letter. In part, he did indeed wish to reveal his possession of knowledge while simultaneously keeping it for himself, just as the purveyors of secrets did – and Kepler seemed quite aware of the part his correspondent had chosen to play.

But Harriot's posture as an alchemist was not merely an expedient role. As Eriksen had informed Kepler, his research into optics and refraction did indeed seem to be connected with alchemical interests. In fact, for Harriot, his optical researches really *were* a species of alchemical experimentation, to which it was natural enough to apply the modes of alchemical concealment and allegory. This is not the conventional interpretation of his optical research. Harriot was certainly as deeply engaged as Kepler was in studying and extending the ancient and medieval optical tradition. Like Kepler and his predecessors, he was interested in optics for its intrinsic interest and also because it promised to resolve questions crucial for astronomy, such as the quantity of atmospheric refraction. But, in addition to

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[59] Ibid., pp. 172–3, letter 497 dated 13 July 1608, p. 172: 'Si illae assumptiones et rationes tibi satisfaciunt, miror, nobis autem non item … Nullam Dyaphanitatem agnosco nisi ratione vacui.'

[60] Ibid.: 'Quae duo olea, cuilibet Alchimistae vel distillatori, notissima sunt.'

[61] Ibid., p. 250: 'Moveram aliqua de Oleo terebinthi, non ut Chymicus, sed ut spectator.'

these, there was a quite different – and novel – impulse behind Harriot's study of optics, as I have shown above in my analysis of Harriot's atomism and its role in his optical theory. Refraction promised to reveal the very structure of matter: the behaviour of a ray of light, wending its tortuous path through a piece of glass or crystal, made evident the discontinuous and indivisible nature of reality. Like alchemy, refraction could lead one into the very 'doors of the house of nature'. One might also say, a little more fancifully, that the evasive and inscrutable trajectory of light through matter seemed to parallel the ambiguity and secrecy of the goldmaker's art, as well as Harriot's own attempts to side-step and misdirect Kepler's questions in his letters.

What is more, Harriot did indeed perform alchemical experiments, although until recently they have been treated as an aberrant episode in an entirely 'rational' scientific career.[62] His most elaborate alchemical experiments took place in 1599 and 1600, during the very period in which he also discovered the law of refraction. In his alchemical work, recently analysed by Stephen Clucas, Harriot had as many as 33 simultaneous experiments going at once, all of which he observed and recorded with careful weighing, diagrams, timing and so forth – in short, demonstrating the same 'mania for quantification' (as Clucas puts it) that he exhibited everywhere in his work, including his optics. The object of the experiments was to isolate the four elements in their purest form, so that he might examine the fundamental constituents of ordinary matter. Thus, alchemy would break down matter in order to reveal its ingredients. But, as we have seen, for Harriot, optics (and refraction in particular) promised to uncover the arrangement and order of the basic constituents of matter. It is no coincidence, then, that in introducing himself to Kepler in 1606, Harriot had Eriksen tell Kepler of his accomplishments both as an optician *and* as an alchemist.

Considered in themselves, Harriot's optical papers remind one little of alchemy. There are traces here and there of the atomic theory of matter that Harriot was exploring via alchemy and refraction (as shown above), but hardly a sign of the Hermetic obscurity he adopted in his correspondence with Kepler. But there is one paper that does make an alchemical connection explicit. In a record of refraction experiments performed in 1613, Harriot again confirmed what he had told Kepler: that several transparent substances of very different specific gravities had the same refractive properties.[63] On this page, however, he separated the transparent substances into two groups. One group comprised glass, amber and gum – these substances were said to be equal 'by my observation'. The other group – crystal and salt – Harriot had also determined to be equal by observation, but also 'docente Trismagisto' – *as Trismegistus teaches*. Beneath, he noted that 'the refraction of gum arabick, yellow amber, christal, sal gemma, is al one' (thereby identifying

[62] See S. Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', in Fox (ed.), *Thomas Harriot*, pp. 93–136, esp. pp. 104–5; and Shirley, *Thomas Harriot*, pp. 268–87 (an account of which Clucas is critical).

[63] BL Add. MS 6789, f. 167r: 'Observationes habitae apud Sion December 30 1613.'

more precisely the salt used in his experiments: *sal gemma*, or large crystals of rock salt, perhaps grown in the laboratory).

Harriot's reference is, on the face of it, mystifying. What could Hermes Trismegistus have to say about angles of refraction? Naturally, this is not a reference to any of the philosophical Hermetica (which, I have argued above, Harriot knew well enough to draw upon in his letter to Kepler). But it may be possible to find a plausible reference in one of the innumerable *alchemical* works which circulated under this mythical name in the Middle Ages and the Renaissance. One likely candidate is a fairly common Hermetic tract entitled 'De salibus et corporibus', written in the thirteenth century.[64] In his survey of the various salts useful in alchemy, the author describes *sal gemma* as a substance 'of the color of crystal'.[65] This is a tenuous connection and it is certainly possible that Harriot was referring to an entirely different Hermetic tract. It may be significant, however, that in his second letter to Kepler, he asked Kepler to add to the table he had previously sent an observation of the refraction of *sal gemma.* This substance, he added, was well known 'in the laboratory' (*in officinis*), and in its density and refractive properties was indistinguishable from crystal.[66] It may be that even in 1608 he was aware of the 'hermetic' text on salts that he would cite in 1613.

Conclusion

In a long dedicatory poem published in 1598, Harriot's friend George Chapman, the poet and great translator of Homer, looked forward to a time when Harriot's 'writings' would 'breake forth like easterne light', dispelling the mists of 'error[']s Night'.[67] The poem, moreover, is strewn with cryptic references to Harriot's scientific investigations, in which Harriot seems to be investigating matter both chemically and optically. In the very opening lines of the poem, Chapman addressed himself 'To you, whose depth of soule measures the height/And all dimensions of all workes of weight'.[68] While 'workes of weight' may mean no more than 'important subjects', Chapman may also have meant to allude playfully to measurements of specific gravity that Harriot was conducting at that very time. Later, having anticipated the publication of Harriot's discoveries, Chapman imagined that all philosophical squabbling would then cease, since Harriot's

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64 On this tract, see L. Thorndike, 'Seven salts of Hermes', *Isis*, 16 (1930), 187–8.

65 Ibid., p. 188: 'Sal gemma qui est coloris cristallini …'

66 Kepler, *Briefe. 1607–1611*, p. 173.

67 On this poem appended to Chapman's *Achilles shield*, see S. Clucas, 'Mathematics and humanism in Elizabethan England', *Journal de la Renaissance*, 4 (2006), 303–18, at 308–9. The poem itself is found in G. Chapman, *Achilles shield* (London, 1598), sigs D1v–D4r, and is entitled 'To my admired and soule-loved friend Mayster of all essential and true knowledge, M. Harriots'. The passage cited here is at sig. D3r.

68 Chapman, *Achilles shield*, sig. D1v.

rivals would see ‘Nature made all transparent, and her hart/Gripte in thy hand, crushing digested Art/in flames unmeasurde, measured out of it’.[69] In this odd, dense description, Chapman seemed to connect Harriot’s ‘measuring’ of nature (by specific gravities?), his analysis of ‘nature’s heart’, or innermost structure, through crushing and digestion in fire (an apparent reference to alchemical processes), and the *transparency* of nature, not implausibly an allusion to his contemporaneous experiments on the passage of light through transparent bodies. He echoed Harriot’s own desires to look into the hidden interiors of his things with his wish that Harriot might have eyes acute enough ‘to pierce/Into that Chaos whence this stiffled verse/By violence breaks’ – to gaze through Chapman’s body into his soul.[70]

Chapman’s impassioned, even ecstatic heralding of Harriot’s discoveries were not enough to move him to emerge from obscurity. If he had intended his poem to be a spur to action, he was as unsuccessful as that other great admirer of Harriot, William Lower. Kepler’s final letter to Harriot arrived some months before Lower’s exhortation, which was quoted at the beginning of this article. Harriot had surely been considering whether to reply – and whether, more importantly, to release his genuinely momentous discoveries in optics to a wider world, as he had surely intended when he had first contacted Kepler. This was the closest he had come to introducing himself to the European republic of letters, and Lower, at least, was not willing to let the chance escape: ‘keepe correspondence with Kepler’, he urged his friend, in the last line of his letter. Harriot, who perhaps by this time could not see how to continue a correspondence that he had so tangled in hermetic obscurity, did not send Kepler a reply.

69 Ibid., sig. D3r.

70 Ibid., sig. D2r.

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Chapter 3

Reconstructing Thomas Harriot's Treatise on Equations[1]

Jacqueline Stedall

The twentieth-century mathematician André Weil wrote that engaging with the history of mathematics is like trying to reconstruct the ocean floor from the occasional islands that protrude above the surface.[2] That is a good description. More often, though, I think it is like trying to fit together a very large jigsaw in which most of the pieces are missing and one is not allowed to look at the picture on the box. One always hopes, of course, that some new and vital piece will turn up, but one knows all too well that it may not. This chapter is about putting together one small part of the jigsaw of Harriot's mathematics, a self-contained treatise of about 140 pages, which I have called the 'Treatise on equations'. I will explain how I reassembled it from pages scattered throughout the manuscripts and I will discuss how Harriot came to write it, what is in it and what happened to it afterwards. In doing so, I will venture a little way into some technical mathematics and make no apology for this, for there is no other way to get to the heart of Harriot's work. But I will do my best to make it as painless as possible.

Harriot's 'Treatise on equations'

We do not know when Harriot first met Nathaniel Torporley (1564–1632), who was at Oxford a little after him, but Torporley was undoubtedly the most mathematically able of Harriot's friends. For a while he was secretary or amanuensis to François Viète, and it was almost certainly through Torporley that Harriot became so intimately familiar with Viète's mathematics, which was otherwise little known

[1] This chapter was originally delivered as a paper in 2002, near the beginning of the decade of lectures in this volume. At that time it was reporting 'work in progress'. The findings discussed then have since been published, and there has subsequently been a good deal of further research into Harriot's mathematics, much of it made possible by the opening up of the Macclesfield Collection in the Cambridge University Library in 2003. This chapter reflects the content and structure of the Harriot Lecture of 2002 but has been updated to include more recent discoveries and conclusions.

[2] A. Weil, *Number theory. An approach through history from Hammurapi to Legendre* (Boston, 1983), p. 3.

outside of Viète's immediate circle of acquaintance. We have an important letter from Torporley to Harriot, written on the eve of his first meeting with Viète in Paris:

> I am gathering up my ruined wittes, the better to encounter that French Apollon: if it fortune that either his courtsie or my boldnes effecte our conference; tomorrow beinge the daye, when I am appoynted by his Printer, as litle Zacheus to climbe the tree, to gayne a view of that renoumned analist. What after followes in [his] presence I hope shortly to relate …[3]

Jon Pepper dated the above letter to 1586, but I suggest it was written several years later, because Viète's books were published in Paris only after 1594. The reference to Viète as the 'French Apollon' points to an even later date, since Viète's *Apollonius gallus* was published only in 1600. This date is consistent with other evidence that Harriot made a serious study of Viète's work from 1600 onwards.[4]

Viète's influence on Harriot was profound, and so we need to know something about the new vision of mathematics that Viète was pursuing. Thoroughly familiar with the writings of Apollonius, Pappus and Diophantus, Viète recognized as no-one had before him that the techniques of algebra could be used to complete or solve some of the more intractable problems of classical mathematics. Indeed, for Viète, algebra was nothing less than the 'analytic art' by which old problems could be understood and new theorems found. Dazzled by the possibilities he foresaw, he claimed, with unbounded optimism, that the analytic art could handle the greatest problem of all: *Nullum non problema solvere*, to leave no problem unsolved.[5]

In order to solve every problem in mathematics, however, one has to be able to solve whatever equations might arise. In the sixteenth century, this meant solving what are now called 'polynomial equations', that is, quadratics (with a square term), such as:

$$x^2 + x = 20$$

or cubics (with a cube term) such as:

$$3x^3 + 5x + 16 = 16x^2$$

[3] Cited in J.V. Pepper, 'A letter from Nathaniel Torporley to Thomas Harriot', *British journal for the history of science*, 3 (1967), 285–90.

[4] For Harriot's notes on the various mathematical treatises of Viète and their dating, see J. Stedall, 'Notes made by Thomas Harriot on the treatises of François Viète', *Archive for history of exact sciences*, 62 (2008), 179–200.

[5] F. Viète, *Opera mathematica* (Leiden, 1646), p. 12.

or quartics (with a fourth power) such as:

$$x^4 + 20x^2 + 100 = 80x$$

All these examples are from Cardano's *Ars magna* of 1545, which Viète almost certainly knew. Quadratic equations are easy to solve, but cubics and quartics are not.

Viète wrote two treatises on solving equations: one theoretical, the other practical. The theoretical text, *De aequationum recognitione et emendatione* (*On the recognition and transformation of equations*), set out the known methods of solving equations up to the fourth degree, but was not published until 1615. The practical text, *De numerosa potestatum resolutione* (*On the numerical solution of equations*), was published in Viète's lifetime and offered something quite new. Here Viète took equations that could be solved only with difficulty, or not at all, by standard methods and showed how numerical solutions could be found to whatever degree of accuracy was required, exactly the kind of thing a computer would now be asked to do. Numerical methods of a similar kind had first been devised by Islamic mathematicians in the twelfth century, but Viète was the first European to teach them. Whether he learned them from earlier texts or rediscovered them for himself is an intriguing and unanswered question, but not one that need detain us here.

De numerosa potestatum resolutione consists essentially of a set of worked examples. Problems 1–9 are concerned with what Viète called 'powers positively affected', that is, equations where the lower degree terms are added to the higher, as in:

$$x^3 + 30x = 86{,}220{,}228$$

Problems 10 to 15 are concerned with 'powers negatively affected', where the lower degree terms are subtracted from the higher, as in:

$$x^3 - 116{,}620x = 352{,}947$$

Finally, Problems 16 to 20 are concerned with what Viète called 'avulsed powers', where a higher power is subtracted from a lower power, as in:

$$65x^3 - x^4 = 1{,}481{,}544$$

All these examples are Viète's.

Amongst Harriot's manuscripts in BL Add. MS 6782, there is a distinctive run of pages marked c.1) to c.18) containing problems numbered 16 to 20. It is clear that these are Viète's 'avulsed power' problems and that Harriot reworked all of them in his own notation. Meanwhile, amongst the Petworth papers is a section marked 'Harriot's Papers: Algebra'. In the modern ordering the pages run in reverse from b.12) to b.1) and they contain Problems 15 to 10, the 'negatively affected powers'. It therefore seemed that there should also be a Section (a) containing Problems

1 to 9, the 'positively affected powers'. In BL Add. MS 6782, there are sheets containing problems numbered 1 to 6 and they are indeed Problems 1 to 6 from Viète's *De resolutione*. There is no letter (a) on these sheets, however, so it seems that Harriot did not start to letter his pages until he began Section (b). Nevertheless, I have taken the liberty of calling these pages 'Section (a)'. Problems 7, 8 and 9 are not included, and it may be that they have been lost, but I think it more likely that Harriot never wrote them out. They are concerned with fifth- and sixth-degree equations, which are laborious to write out but add little to what has already been shown in the first six problems. Assuming that Harriot worked only on Problems 1 to 6, we therefore have, divided between London and Sussex, a complete version of Harriot's Sections (a), (b) and (c), containing his reworking in his own notation of Viète's *De resolutione*.

But Harriot did more than that. Amongst his papers it is also possible to identify three further sections, lettered (d), (e) and (f), where he began to move beyond Viète into his own research. Section (d) is headed 'De generatione aequationem canonicarum' ('On the generation of canonical equations'). To understand what is in it, we need to recall the method of solving quadratic equations by factorization. Suppose, for instance, we want to solve the equation:

$$x^2 + 6x - 40 = 0$$

By trial and error we can rewrite this as:

$$(x - 4)(x + 10) = 0$$

Now we can see that either $x - 4 = 0$ or $x + 10 = 0$ and so $x = 4$ or $x = -10$. The beauty of this method is that the same principle extends to cubic or quartic or higher degree equations, so that we can write, for example:

$$x^3 - 6x^2 + 5x + 12 = (x + 1)(x - 3)(x - 4) = 0$$

The idea of studying equations in this way was Harriot's, and it is of profound significance because it enables mathematicians not just to solve equations but also to look inside their structure. In the quadratic equation above, for instance, we can see not only that $-40 = 4 \times -10$ and $6 = 4 + (-10)$ but also why the coefficients -40 and 6 *must* be of this form.

In his Section (d), Harriot did precisely what has just been described: systematically constructing equations as products of factors. Like any good investigative mathematician, he started simply with quadratic equations, before moving on to cubics and quartics. The equations he built up in this way he called 'canonical'. His aim, it appears, was to create a comprehensive list of canonical equations, so that given any new equation he could compare it to one of his canonicals, and thereby immediately know something about its solutions.

Sections (e) and (f) can be described quite briefly. Both are entitled 'De resolutione per reductionem' ('On solving equations by reduction'). Section (e) is a systematic treatment of cubic equations. Section (f) is a similar treatment of quartic equations. It is lengthy, and Harriot seems to have written out different bits of it at different times, so that it is not easy to reconstruct it from his papers. There are two runs of sheets labelled f.1) to f.7), for example, another run labelled f.8) to f.17), a single run of eight sheets and two runs of four all marked just with f), together with further sheets of related rough work scattered randomly through the manuscripts. The unifying factor, however, is that all the equations in these sheets are quartics, and it is clear that Section (f) was meant to be a systematic treatment of quartics just as Section (e) was a systematic treatment of cubics.

If we now take these six sections together, we have something that it is not at all apparent when one first looks at the confusion of the manuscripts: a self-contained, logical and coherent treatise on the structure and solution of equations. None of the pages is dated, but the first three sections of the treatise arise so directly from Viète's *De resolutione* that it is likely to have been written soon after 1600. For convenience I have called it simply Harriot's 'Treatise on equations'.

Neither the section headings nor the above brief description can convey what a pleasing piece of work it is. Harriot, as always, wrote both concisely and effectively. There are very few errors and where they occur, it is usually because Harriot is feeling his way over new ground. There is not enough space here to describe the many subtleties and intricacies of the treatise, but it is an original and beautiful piece of mathematics.[6]

The fate of the 'Treatise on equations'

Harriot's friend William Lower wrote in vain to him in 1610 begging him to publish, amongst other things, his algebra:

> Doe you not here startle to see every day some of your inventions taken from you; for I remember long since you told me as much (as Kepler has just published) that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print. A little before, Vieta prevented you of the Gharland for the greate Invention of Algebra. Al these were your deues and manie others that I could mention; … Let your Countrie and friends injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise workes.[7]

[6] The 'Treatise on equations' is now published in full in J. Stedall, *The greate invention of algebra. Thomas Harriot's treatise on equations* (Oxford, 2003).

[7] J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), pp. 1–2.

Despite Lower's entreaties, Harriot never did publish his work on equations or any other of his mathematical or scientific findings. However, just a few days before he died in 1621 he made a will and in it gave particular attention to his mathematical papers:

> I ordaine and Constitute the aforesaid NATHANIEL THORPERLEY first to be Overseer of my Mathematical Writings to be received of my Executors to peruse and order and to separate the chief of them from my waste papers, to the end that after he doth understand them he may make use in penning such doctrine that belongs unto them for public uses as it shall be thought Convenient by my Executors and him selfe.[8]

In 1621, when Harriot died, Torporley was vicar of Salwarpe in Worcestershire, but in the following year he resigned his post, presumably to work full-time on Harriot's papers. We know that during the next 10 years, before his own death in 1632, he completed two pieces of work based on Harriot's manuscripts and sketched out a third. These are described in more detail below. He spent the last years of his life at Sion College, then a home for retired clergymen, and after his death his surviving papers were deposited in Sion College Library. There they remained for almost four centuries, until 1996, when the contents of the Library were transferred to Lambeth Palace. Thus, some of the most important contemporary documentary evidence relating to Harriot's work is now to be found amongst the Church of England archives.

Items written by Torporley relating to Harriot's mathematics were:

1. *A treatise based on Harriot's method of interpolation by constant differences.* Torporley headed it 'Na. To. CONGESTOR … eodem se forte resolvit CONIECTOR …'. He dedicated the completed copy to the Earl of Northumberland on 5 October 1627. It is listed in the Sion College Benefactors' Book as 'Congestor analiticus cui accessit conjector'. However, some time during the seventeenth century, the treatise was removed from Sion College, by whom we do not know. Eventually it reached the hands of John Collins and was left, along with many other letters and papers belonging to Collins, to William Jones, tutor to the son of the Earl of Macclesfield. The 'Congestor' is therefore now part of the Macclesfield Collection, which was purchased in 2000 by Cambridge University Library.[9]

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8 R.C.H. Tanner, 'Thomas Harriot as mathematician: a legacy of hearsay', *Physis*, 9 (1967), 235–47.

9 N. Torporley, 'Congestor … [et] … coniector', now CUL 9597/17/28. For discussion of the contents of this document, see J. Beery and J. Stedall, *Thomas Harriot's doctrine of triangular numbers. The 'Magisteria magna'* (Zurich, 2009).

2. *A discussion of Harriot's work on prime numbers and Pythagorean triples*. This is now held in Lambeth Palace Library and is almost certainly the first part of the item listed in the Benefactors' Book as 'Problemata varia de analiticis'. Unfortunately, Tanner mistakenly identified this document as the 'Congestor analiticus'. This misconception persisted in the literature until the true 'Congestor' became publicly available in Cambridge three years after the purchase of the Macclesfield Collection.[10]
3. *A synopsis of Harriot's work on equations*. In Torporley's manuscripts this follows immediately after (2) and thus appears to form the second part of the 'Problemata varia de analiticis'.[11] Elsewhere I have referred to this document as Torporley's 'Summary' and for consistency will also do so here.[12] Its contents will be described in more detail below.
4. The 'Corrector analyticus', which Torporley wrote towards the end of his life in response to the publication of the *Praxis*.[13] This too will be described more fully below.

Despite this considerable output, it seems that Torporley was not allowed to complete all that he wanted do to with Harriot's manuscripts. There are hints of disagreement between Torporley and Harriot's executors, Thomas Aylesbury and John Protheroe, but the reasons for this are not clear. In 1631 Torporley (then in his late sixties) described himself as an old man approaching death. Even in his younger days he was one of the most long-winded writers imaginable and it is possible that Aylesbury and Protheroe feared he was incapable of doing what was required in any foreseeable length of time and so invited Walter Warner to help out.

There is no editor's name on the title page of the *Artis analyticae praxis*, the edition of Harriot's algebra eventually published in 1631, perhaps because his executors knew that by excluding Torporley they had contravened the terms of Harriot's will. Nevertheless, it was common knowledge at the time that the person primarily responsible for the book was Warner. Over many years of friendship and association with Harriot, Warner had probably learned a good deal about Harriot's mathematics but was not himself a deep mathematical thinker. Indeed, Jan Prins describes him as 'a not too clear-thinking minor philosopher', which seems an

[10] 'Problemata varia de analiticis' (first part), Sion College MS Arc. L.40.2/L40, ff. 1–34v. The misidentification of this document with the 'Congestor' stems from R.C.H. Tanner, 'Nathaniel Torporley's "Congestor analyticus" and Thomas Harriot's "De triangulis laterum rationalium"', *Annals of science*, 34 (1977), 393–428.

[11] 'Problemata varia de analiticis' (second part), Sion College MS Arc. L.40.2/L40, ff. 35–49v.

[12] Stedall, *The greate invention of algebra*, p. 24.

[13] 'Corrector analyticus', Sion College MS Arc. L.40.2/E.10, ff. 7–12.

astute enough judgement.[14] Warner certainly never understood either Harriot's mathematics or Viète's as well as Torporley did, as we can see by examining what he did with some of Harriot's material.[15]

In Section (d) of his 'Treatise on equations' Harriot had treated each new equation in a unified way, exploring various aspects of one equation before moving on to the next. Warner, however, chose a different way of arranging things, dispersing material on each equation across Sections 1, 2 and 4 of the *Praxis*. He also changed the ordering of the equations from one section to another, so that in the *Praxis* the structure and coherence of Harriot's Section (d) is completely lost. Harriot's Section (e), or as much as Warner selected from it, is scrambled through Sections 5 and 6 of the *Praxis*. Section (f) is represented at length but without the key material at the beginning that explains what all the rest is about; we therefore find in the *Praxis* some 30 examples of removing the cube term from a quartic with no clue as to why one should need to do it.

The *Praxis* was the one book upon which Harriot's reputation rested. It succeeded in making his work on equations known to a wider public, but at the same time was in many ways a travesty of his original intentions. No-one knew that better than Torporley, who was shocked by it and vented his criticisms in the piece entitled 'Corrector analyticus' (see (4) above). Translated into English, its opening words are:

> An Analytic Correction of the posthumous work of Thomas Harriot
>
> As an exceptional mathematician, one who very seldom erred,
> As a bold philosopher, one who more often erred,
> As a mere human, one who conspicuously erred.
>
> For the more trustworthy refutation of the pseudo-philosophic atomic theory revived by him, and other strange notions deserving reprehension and anathema. A compendious warning with examples by the aged and retired Nathaniel Torporley.

This preamble suggests that Torporley was going to write against Harriot's scientific and philosophical theories, but what actually followed was a long, rambling, and bitter diatribe against the editors of the *Praxis*. Torporley's Latin is horrible and we are still in need of a full translation of this and his other writings. There is one part of the 'Corrector', however, that is not difficult to understand, where Torporley set out very clearly what he thought the editors of the *Praxis*

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[14] J.L.M. Prins, 'Walter Warner (ca 1557–1643) and his notes on animal organisms' (PhD thesis, Utrecht University, 1992), p. xviii.

[15] An English translation of the *Praxis* is now available in M. Seltman and R. Goulding, *Thomas Harriot's Artis analyticae praxis. An English translation with commentary* (New York, 2007).

should have done. In it he referred specifically to sections that he called (d), (e) and (f), and it is clear from his descriptions that these are Sections (d), (e) and (f) identified above. He then referred to a section 'supposed' (a) (recall that there was no actual letter (a) on the sheets) and then to sections (b) and (c), for which he cited specific sheet numbers and examples. Comparing his notes with the contents of the manuscripts, it is clear that he was giving a complete and almost entirely accurate description of Harriot's 'Treatise on equations'.

This was not the only document in which Torporley recorded the material he thought should be included in a posthumous edition of Harriot's algebra. His 'Summary' (see (3) above) is a condensed version of the entire 'Treatise on equations', in which he reduced the contents of more than 200 of Harriot's pages into a 20-page document. When I first saw this manuscript in Lambeth Palace Library in April 1999, led to it by an obscure footnote in a paper by Tanner, I recognized that the sheets Torporley listed were still extant and knew that I should attempt to complete what I believe he himself had hoped to see.[16] The conclusion of this part of the story is that my reconstructed edition of Harriot's 'Treatise on equations' was published by Oxford University Press in 2003. It was hoped as long ago as the 1790s that the Press would print some of Harriot's material so it is altogether fitting that it should finally have fulfilled that expectation two centuries later.[17]

Harriot's later influence

Questions have been asked repeatedly about Harriot's influence and the significance of his work, however original, given that it was not published. In addressing such questions it must be borne in mind that in the first half of the seventeenth century, mathematical publication in England was the exception rather than the rule. Instead, mathematical ideas were exchanged freely amongst the small number of people who were interested in them by means of letters, manuscripts and conversations.[18]

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[16] The reference that led me to the Torporley manuscripts is in R.C.H. Tanner, 'The alien realm of the minus: deviatory mathematics in Cardano's writings', *Annals of science*, 37 (1980), 159–78. I later discovered that Tanner had also identified and listed the manuscript pages that Torporley had described, though she does not appear to have recognized the existence of a self-contained treatise; see R.C.H. Tanner 'Henry Stevens and the associates of Thomas Harriot', in J.W. Shirley (ed.), *Thomas Harriot. Renaissance scientist* (Oxford, 1974), pp. 91–106.

[17] '… it is with pleasure I can announce, that they are in a fair train to be published: they have been presented to the university of Oxford on condition of their printing them': C. Hutton, 'Algebra', in *A mathematical and philosophical dictionary*, 2 vols (London, 1795–6), vol. 1, p. 586.

[18] For evidence of the persistence of Harriot's ideas by word of mouth, see Beery and Stedall, *Harriot's doctrine of triangular numbers*.

There is evidence that Harriot's manuscripts remained in circulation for up to 30 years after his death.

We know that following the publication of the *Praxis*, Aylesbury and Warner intended to publish more of Harriot's work and hence, rather than returning the papers to the Earl of Northumberland as Harriot's will had stipulated, Aylesbury kept the papers in his own possession. By 1639 John Pell had also become interested in Harriot's algebra and Samuel Hartlib noted that Aylesbury was still holding the papers: 'Sir Thomas Alesbury promised to let [Pell] have Harriot's papers but hee did solve [the problems] without them.'[19] Twelve years later, Warner was dead, Pell was in the Netherlands and Aylesbury was living in exile in Antwerp, but it seems that Harriot's papers were still with Aylesbury. On a visit to Antwerp in 1651, Sir Charles Cavendish wrote to Pell: 'Sr. Th. Alesburie remembers him to you and desires to knowe if you would be pleased to shew the use of Mr. Hariots doctrine of triangulare numbers; which if you will doe he will send you the originall.'[20] Harriot's 'doctrine of triangulare numbers' is the treatise otherwise known as his 'Magisteria magna'. This is now held with his other mathematical papers in the British Library and there is no reason to suppose that it was ever separated from the rest. It therefore seems that 30 years after Harriot's death, Aylesbury was still in possession of Harriot's manuscripts and indeed considered them important enough to take with him into exile.

Charles Cavendish, author of the letter to Pell, was more important as a disseminator of mathematical ideas than has generally been recognized. In the 1620s and 1630s he brought the mathematics first of Viète and then of Bonaventura Cavalieri to William Oughtred in England. He also took ideas in the other direction, from England to his acquaintances in Paris.[21] This brings us to the difficult and much-debated question of whether Descartes knew of Harriot's algebra when he wrote *La géométrie*, published in 1637. Without new evidence there can be no definitive answer. Descartes, like Harriot, devised a notation with lower case letters and, also like Harriot, based his study of equations on the principle of factorization. One can easily see why his contemporaries thought he might have gleaned such ideas from Harriot. Indeed, the French mathematician Jean Beaugrand, who had edited some of Viète's work, suspected that he detected in *La géométrie* the influence of both Viète and Harriot, but Descartes denied that he had read anything of either. This may have been true; nevertheless, it seems almost inconceivable that Descartes, who had lived in Paris and corresponded

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[19] S. Hartlib, *Ephemerides*, 1639, University of Sheffield Hartlib Papers, 30/4/9B.

[20] Cavendish to Pell, [26 September] 6 October 1651, BL Add. MS 4278, ff. 321–2; reproduced in N. Malcolm and J. Stedall, *John Pell (1611–1685) and his correspondence with Sir Charles Cavendish. The mental world of an early modern mathematician* (Oxford, 2005), p. 584.

[21] See, for example, the story of a meeting between Cavendish and Roberval recounted in J. Wallis, *A treatise of algebra historical and practical* (London, 1685), p. 198; reproduced in Stedall, *The greate invention of algebra*, pp. 28–9.

with Mersenne, was not to some extent familiar with the mathematical ideas that were circulating there.

In England there was lingering recognition for many years that Harriot had done something rather remarkable. Pell, who had learned of Harriot's work directly from Aylesbury and Warner, later told John Collins, in words that echoed those of Lower 60 years earlier, that Harriot was 'so learned, that had he published all he knew in algebra, he would have left little of the chief mysteries of that art unhandled'.[22] John Wallis, Savilian Professor of Geometry at Oxford, whose account of Harriot's algebra in his *Treatise of algebra* was in turn gleaned from Pell, also recognized the implications of Harriot's work:

> Besides conveniences in the Notation, Mr. Harriot, as to the Nature of Equations, (wherein lyes the main Mystery of Algebra;) hath made much more improvement. Discovering the true Rise of Compound Equations; and reducing them to the Originals from whence they arise.[23]

Charles Hutton, writing in *A mathematical and philosophical dictionary* a century later, made observations very similar to those of Wallis and commented on the way in which the inner structure of equations became visible through Harriot's work:

> [Harriot] shewed the universal generation of all the compound or affected equations, by the continual multiplication of so many simple ones; thereby plainly exhibiting to the eye the whole circumstances of the nature, mystery and number of the roots of equations; with the composition and relations of the coefficients of the terms; and from which many of the most important properties have since been deduced.[24]

Unfortunately, in the 200 years after Hutton wrote, the attribution of these achievements to Harriot was somehow lost. Later commentators became mired down in arguments about whether or not Harriot allowed negative roots and lost sight of his much greater contributions: (i) his invention of a clear and usable notation, much of which is still in use today; and (ii) his insight into the way polynomials could be constructed as products of simple factors, leading to a new and much deeper understanding of the structure of equations.

In attempting to restore Harriot's original 'Treatise on equations', I see myself as but the latest in a long line of people who have hoped that Harriot and his algebra would eventually get the recognition they deserve. In the seventeenth century alone, William Lower, Nathaniel Torporley, Thomas Aylesbury, Walter Warner, John Pell and John Wallis all tried to see justice done to Harriot and

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22 Collins to Vernon (c. 1670), in S.P. Rigaud (ed.), *Correspondence of scientific men of the seventeenth century*, 2 vols (Oxford, 1841), vol. 1, pp. 152–3.

23 Wallis, *Treatise of algebra*, p. 128.

24 Hutton, 'Algebra', in *A mathematical and philosophical dictionary*, vol. 1, p. 91.

his mathematics. Of these, Torporley is now one of the least remembered. Yet Torporley's notes, written in the final years of his life, now constitute the most important documents we possess, apart from Harriot's manuscripts themselves, in relation to Harriot's algebra. They offered me both inspiration and confirmation for all that I have subsequently tried to do in relation to Harriot's work on equations.

Chapter 4

Harriot on Combinations[1]

Ian Maclean

This chapter will look at Thomas Harriot's interest in combinations in three contexts – language, natural philosophy (the question of atomism) and mathematics – in order to assess where to situate him in the range of occult and scientific mentalities associated with the late Renaissance. At his death in 1621, he left many pages of mathematical workings and drafts, but relatively little discursive prose; this fact has been linked to the privacy with which he surrounded his work and his notorious reluctance to publish his discoveries.[2] Hilary Gatti has even gone so far as to suggest that his use of symbols and diagrams in his manuscripts reflects a 'distrust of words'.[3] Whether or not this is the case, it means that much has to be made out of a few not always legible gnomic sentences, which are often subject to almost contradictory readings.

I shall give one example here, as it is relevant to this chapter: it is a passage from the letter Harriot wrote in 1615 to his physician Théodore Turquet de Mayerne (1573–1655). After a recital of his symptoms (consistent with a cancer induced by smoking, which he had acquired as a habit while in the New World), he writes:

> Think of me as your most affectionate friend. Your interests therefore are as mine. My health will be your glory too, but through the Omnipotent who is the author of all good things. As I have said from time to time, I believe in three things. I believe in one almighty God; I believe in the art of medicine as ordained by Him; I believe in the physician as His minister. My faith is sure, my hope is firm. I wait patiently for everything, in its own time, according to His providence. Let us act resolutely, battle strenuously, and we shall win. The world's glory passes away. Everything will pass away; we shall pass, you will

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[1] Reproduced with kind permission of the Société mathématique de France from *Revue d'histoire des mathématiques*, 11 (2005), 57–88. I should like to thank Jackie Stedall for her help in the preparation of this chapter.

[2] See Sir Thomas Lower's letter to Harriot dated 6 February 1609/10, quoted in G. Batho, 'Thomas Harriot's manuscripts', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 286–97 (287).

[3] H. Gatti, 'The natural philosophy of Thomas Harriot', in Fox (ed.), *Thomas Harriot*, p. 66.

> pass, they will pass. I wrote to your apothecary for the pills. Perhaps I will receive one dose before Advent.[4]

As Hugh Trevor-Roper notes, this is a strange passage to find in a letter to one's physician, and it invites comment. Mayerne, as is well known, was a Montpellier-trained doctor who was sympathetic to Paracelsian ideas.[5] Two contexts occur to me which might throw some light on the sense to be attributed to the passage. The former is the following statement by Jean Hucher (d. 1603), Mayerne's colleague when he was at Montpellier, that:

> The most high and great God, the lord of all of nature, freely administers, impels, hastens, delays, hinders or altogether prohibits the forces, actions and effects of nature … therefore Aristotle's disputations about chance and fortune as two unknown efficient causes are rightly laughed off the stage by pious men, for God is alone the author of all spontaneous events and their contingency.[6]

This might suggest that the Harriot who was 'waiting patiently for everything, in its own time, according to God's providence', concurred in a Montpellerian belief that the doctors were no more than vehicles through whom divine will is implemented (a view consistent with Paracelsianism and implicitly hostile to the claims of rational Galenic medicine).[7]

The second context comes from a book published in the same year as Harriot's letter by the physician and proto-chemist Andreas Libavius (1550–1616) entitled the *Examination of the new [Paracelsian] philosophy, which is opposed to the old and seeks to abrogate it*; Harriot owned at least one of Libavius's other works

[4] BL Add. MS 6789, f. 446v (letter to Théodore Turquet de Mayerne, 1615): 'cogita de me tanquam tui amantissimo. Tua res igitur [S. Mandelbrote, 'The religion of Thomas Harriot', in Fox (ed.), *Thomas Harriot*, p. 247, reads 'auspicat'] sicut et mea. Mea salus erit etiam tua gloria sed per omnipotentem qui omnium bonorum est author. Ut aliquando Dixi, tripliciter credo. Credo in Deum omnipotentem Credo medicinam ab illo ordinatam, Credo medico tanquam illius ministro. Fides mea certa spes firma. Expecto tamen cum patienta omnia suis temporibus [Harriot adds 'suo tempore' as an alternative above these words] secundum illius providentiam. Agendum serio, pugnandum strenue, sed eius nomine, et vincemus. Sic transit gloria mundi. Omnia transibunt, nos ibimus, ibitis, ibunt. Scripsi ad tuum pharmocopaeum pro pillulis fortasse unam dosim capiam ante adventum.'

[5] H. Trevor-Roper, 'Harriot's physician: Théodore de Mayerne', in Fox (ed.), *Thomas Harriot*, pp. 48–63.

[6] J. Hucher, *De prognosi* (Lyon, 1602), sig. **6v: 'Deus optimus maximus totius naturae dominus, vires actiones et effectus eiusdem libere administrat, impellit, urget, tardat, interpellet aut omnino prohibet. Merito igitur Aristotelis de casu et fortuna tanquam effectricibus duabus causis ignotis, disputationes a piis viris exploduntur; cum omnium spontaneorum casuum solus Deus sit author, eorumque contingentiae.'

[7] See I. Maclean, *Logic, signs and nature in the Renaissance. The case of learned medicine* (Cambridge, 2001), pp. 87–90.

(a pamphlet against the Rosicrucians) and it is possible that he knew this one. In it, Libavius argues that to philosophize in a Christian way is to follow Aristotle, not magic, Cabbalah, alchemy, astrology, chiromancy, or, of course, Paracelsus; that conventional Aristotelianism represents the order of God; and that rational medicine (as opposed to Paracelsianism), which looks upon itself as the 'minister and corrector of nature', is a gift of God.[8] The similarity in terms and sentiment with Harriot's letter is pretty clear, but its implication is the opposite of the meaning which can be derived from a comparison with the Hucher text. I do not doubt that Harriot was writing to please Mayerne, but it is difficult to know what he was trying to say: the first of my contexts would suggest that Harriot was recognizing the Paracelsian mission of his physician as a passive channel of God's grace, and the second that he was stressing his active ministry. I shall return to this choice of interpretations at the end of this chapter, in the context of Harriot's religious beliefs. Before that, I shall place some other enigmatic or elliptical comments by Harriot in contexts, mainly drawn from continental writers quoted by Harriot himself, such as the mathematician Michael Stifel (1487–1567) and the polymath Girolamo Cardano (1501–76).

Historiographical debates

Three recent debates in the historiography of science are relevant to this chapter. The first of these concerns the thesis that there are separate 'scientific' and 'occult' mentalities in the late Renaissance. The latter mentality has been dubbed by W.B. Ashworth, Jr., the 'emblematic world view', according to which the book of nature was believed to be written not, as Galileo was to aver, in the language of mathematics, but in an intricate metaphorical discourse of symbols and emblems whose decoding yielded understanding of the meaning of the cosmos and of human existence.[9] In the anthology of essays entitled *Thomas Harriot. An Elizabethan man of science*, some bracingly different views on this very issue are juxtaposed. The 'occult' or 'emblematic' view owes much to Frances Yates and the discovery she claimed to have made of an elite Christian neo-Platonic humanist intelligentsia in England in the latter years of the sixteenth century, interested in natural magic and humanity's future; one may take as the antipode of this view the claim that at

[8] Andreas Libavius, *Examen novae philosophiae, quae veteri abrogandae opponitur* (Frankfurt, 1615), p. 298: 'ordo Dei est Philosophia quae docetur in Gymnasiis, Scholis, et Academiis, ut et Theologia syncera declarata Augustanae Confessione. Dei donum est medicina dogmatica et aliae artes scientiaeque'. On Harriot's possession of another of Libavius's works, see Mandelbrote, 'The religion of Thomas Harriot', p. 252.

[9] W.B. Ashworth, Jr., 'Natural history and the emblematic world view', in D.C. Lindberg and R.S. Westman (eds), *Reappraisals of the scientific revolution* (Cambridge, 1990), pp. 303–32. See also B. Vickers (ed.), *Occult and scientific mentalities in the Renaissance* (Cambridge, 1984).

the same time there were thinkers with a scientific outlook struggling to break free from 'backward Renaissance thinking'. Naturally, a number of intermediate or variant opinions are also expressed: according to one, Harriot was not an orthodox Christian but an atheist in the sixteenth-century sense of that term; or he was a humanist and atomist beguiled by Giordano Bruno's neo-Platonic hermetic vitalist version of this doctrine; or again, he was 'scientific in one sense but still linked to animistic precepts of Renaissance magic, alchemy and the regrettable concomitant Hermetic traditions of secrecy and concealment ' (the view of Charles Nicholl and to some degree J.W. Shirley); or yet again, the claim made by Stephen Clucas that the dichotomy between scientific and occult is the product of a modern mindset and is inappropriately applied to Elizabethan thinkers.[10] A further possibility not considered is that the dichotomy can indeed be applied to these thinkers, but that their commitment to one or the other side is intermittent, or determined by the matter in hand.

The debate is seen most starkly in the opposition between the figure of Harriot the natural philosopher and Christian on the one hand, and Harriot the mathematician on the other. Here we may catch echoes not only of the debate about social constructivism in scientific thought, and the role of ideological commitments and practical concerns (including subjugation to the wishes of patrons) in the most abstract mathematical speculation, but also of the second debate to which I wish to draw attention: that between Andrew Cunningham and Roger French on the one hand and Edward Grant, David Lindberg and Ronald Numbers on the other as to whether there could be a secular outlook in medieval and early modern Europe – whether there is a continuity between the investigations of natural philosophers and modern scientists or whether 'natural philosophy' has a quite different character (involving necessarily a consideration not only of God's creation but also of the attributes of God Himself). Several of the contributors to the volume *Thomas Harriot. An Elizabethan man of science*, edited by Robert Fox, clearly believe that there is such mental activity as 'thinking like a mathematician' which is perennial, utterly untheological and immediately recognizable: Muriel Seltman writes that 'Harriot's shorthand of mathematics brings the reader today into direct contact with historical mathematics … [A] little idiosyncrasy [of his] takes one to the heart of the mathematician, to give us an emotional fellow-feeling spanning four centuries', and Jim Bennett avers that 'if anything comes over from a look at Harriot's manuscripts it is surely that … he was a mathematician'.[11] I defer to these

[10] S. Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', in Fox (ed.), *Thomas Harriot*, pp. 93–136.

[11] On the constructivism debate, see N. Jardine and M. Frasca-Spada, 'Splendours and miseries of the science wars', *Studies in the history and philosophy of science*, 28 (1997), 219–35. See also R.K. French and A. Cunningham, *Before science. The invention of the friars' natural philosophy* (Aldershot, 1996); and D.C. Lindberg and R.L. Numbers (eds), *When science and Christianity meet* (Chicago, 2003). The battle lines are clearly set out by A. Cunningham and E. Grant in *Early science and medicine*, 5 (2000), 258–300. See

views, but they do not prevent me from asking whether one can detect ideological or other commitments in Harriot's work by drawing comparisons with other Renaissance mathematicians and philosophers who may differ in their approach.

A third debate concerns the relationship of the 'mathematical sciences' on the one hand and 'natural philosophy' on the other in the early modern world. What is at stake here is clearly set out in J.A. Bennett's article 'The mechanics' philosophy and the mechanical philosophy' of 1986.[12] In this he criticizes the distinctions made by T.S. Kuhn and others between empiricists and mechanists and between high and low science for their anachronism, and suggests that the category known as the 'mathematical sciences' as used in the late Renaissance covers all sort of mathematical activity, whether undertaken by learned figures such as Harriot or by artisan instrument makers; there is, according to him, a continuum of mathematical practice which unites the theoretical and the utilitarian at this time. It will be pertinent here to test Harriot's interest in combinations against this characterization of mathematics.

Versions of combination

The combinations to which I shall refer here are to be found in language (the combinations of letters to make words), natural philosophy (the combination of atoms which go to make up matter) and, of course, mathematics. Combinations in mathematics and language are explicitly linked in the tradition of Cabbalah, as we shall see; combinations in language and natural philosophy were explicitly linked in the Middle Ages, as the work of Zenon Kaluza has shown. Kaluza has revealed that there were Parisian philosophers known as 'Epicurei litterales' in the early fourteenth century in Paris who laid claim to 'libertas philosophandi' (in this case, liberation from the sanctioned interpretation of Aristotelian physics) and described matter in anti-hylemorphic terms through a linguistic metaphor (matter is composed of atoms in the same way as words are composed of letters).[13] Epicurean philosophy as then understood was a version of Democritic atomism (known in summary form through various Aristotelian works, notably the *De caelo*, iii.4, 303a 3ff.); it has the possible further implication of the doctrine of

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also M. Seltman, 'Harriot's algebra: reputation and reality', pp. 153–85 (p. 167) and J.A. Bennett, 'Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning', pp. 137–52 (p. 141), both in Fox (ed.), *Thomas Harriot*.

[12] J.A. Bennett, 'The mechanics' philosophy and the mechanical philosophy', *History of science*, 24 (1986), 1–28.

[13] Z. Kaluza, 'Le *De universali reali* de Jean de Maisonneuve et les Epicurei litterales', *Freiburger Zeitschrift für Philosophie und Theologie*, 35 (1986), 465–516. See also Z. Kaluza, *Les querelles doctrinales à Paris. Nominalistes et réalistes aux confins du XIVe et du XVe siècles* (Bergamo, 1988); and L. Bianchi, *Censure et liberté intellectuelle à l'Université de Paris. XIIIe–XIVe siècles.* (Paris, 1999).

purposelessness in nature, which was anathema to Christian theology. I doubt very much whether Harriot and his circle knew of this debate, but they were manifestly aware both of atomism and its implications, and of the notion of the 'freedom to philosophize', as we shall see.

The three areas in which I shall discuss combinations are linked in the wording of the memorial erected by Harriot's patron the Earl of Northumberland in the church in which he was buried, which contains the words:

> he devoted himself to all the whole field of knowledge; he excelled in all things mathematical. philosophical, theological; a most diligent student of truth, and a most pious worshipper of the triune God.[14]

'Things theological' is here ambiguous. I do not think that the Earl meant Christian dogmatic theology, but rather metaphysics in general (the standard edition of Aristotle's works of 1619 refers to his metaphysics as theology); he may even have had in mind Bacon's attenuated version of this – the accumulation of axioms which must precede any enquiry into nature.[15] Theology in its broadest sense is relevant to my first topic: words and their possible mystical associations; my second will be atomism and its place in Harriot's natural philosophy; and my last will be numbers and mathematics. At the end of this chapter I shall venture to comment on the connection between natural philosophy and mathematics in the light of the debates evoked above, and on the description of Harriot as 'a most pious worshipper of the triune God' in the context of the debate about the meaning to be ascribed to the term 'natural philosophy'.

Words

There are two broad schools of thought about words as combinations of letters which would have been known to Harriot: the neo-Platonist and Cabbalistic on the one hand, and the anagrammatological on the other. The former of these unites a tendentious interpretation of Plato's dialogue *Cratylus* which is about names

[14] The inscription on Harriot's memorial in St Christopher le Stocks reads: 'Qui omnes scientias caluit/Qui in omnibus excelluit/Mathematicis, Philosophicis, Theologicis/Veritatis indagator studiosissimus/Dei Trini-Unius cultor piissimus': quoted from J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), pp. 473–4.

[15] Aristotle, *Opera omnia*, ed. G. Duval (Paris, 1619), 'Synopsis analytica doctrinae peripateticae', ii.84(on 'theologia naturalis' as the study of 'ens quatenus ens': a definition possibly derived from the division of metaphysics made by B. Pereira in his *De communibus omnium rerum naturalium principiis et affectionibus* (Rome, 1576), on which see C. Lohr, 'Possibility and reality in Suárez's *Disputationes metaphysicae*', in E. Kessler and I. Maclean (eds), *Res et verba in the Renaissance* (Wiesbaden, 2002), pp. 276–7); and F. Bacon, *The advancement of learning*, ed. M. Kiernan (Oxford, 2000), pp. 76–8.

with Christian interpretations of the Jewish Cabbalah. The *Cratylus* was made available in Latin by Marsilio Ficino in the fifteenth century; it was developed most energetically in the mid-sixteenth century by Jean Daurat, the charismatic humanist tutor of the French group of poets known as the Pléiade who were studying at the Collège de Coqueret of the University of Paris. It claimed (to put it in the words of the most famous of these poets, Pierre de Ronsard) that 'les noms ont puissance et efficace et vertu'; they are moreover linked to the essence of that of which they are the name.[16]

Cabbalah was made known in the Renaissance through the works of Giovanni Pico della Mirandola (1463–94) and Johannes Reuchlin (1455–1522), whose accounts were used in a great deal of subsequent literature; the French writer Blaise de Vigenère (1523–96) linked Cabbalah to cryptology in 1586 in his *Traité des chiffres*. Words are given a very important role in Cabbalistic writings: the world is said to be created out of the 22 letters of the Hebrew alphabet, whose very shapes have significance; each individual letter has two meanings, one open and one hidden; Holy Writ hides all the names of God, and these can be revealed by textual manipulations; Adam named all things under guidance from God and thereby provided things with a path to help them establish their identity; the cabbalist can apply three operations to letters (transmutation, commutation and combination); if he were to produce all possible combinations of Hebrew letters, then the names of all created things would be given (including the secret and unutterable names of God), and man would have equalled God's feat of counting the stars, the grains of sand in the world and all the hairs on the combined heads of humanity.[17] Blaise de Vigenère points out that merely combining all the letters of the Hebrew alphabet (not counting the additional combinations which could be achieved by the application of points) would result in 11,240,025,908,719,680,000 words (he is commendably close to the right answer); the fact that it is beyond man's capacity is a demonstration of the gap which divides the finite from the infinite, the human and the divine.[18] There was some reception in England of these

[16] P. de Nolhac, *Ronsard et l'humanisme* (Paris, 1921); Ronsard, *Sonnets pour Hélène*, ed. M. Smith (Geneva and Paris, 1970), p. 109 (ii.6: 'Anagramme'), line 9; and Smith's notes, pp. 109–10. There is a considerable anagrammatological literature in nearly all countries of Europe at this time.

[17] These comparisons are biblical. See Ps. 147:4, Jeremiah 33:22, Matthew 10:38.

[18] Blaise de Vigenère, *Traicté des chiffres, ou secretes manieres d'escrire* (Paris, 1587), f. 33v (reference to transmutation, commutation and 'accouplemens de lettres'); f. 37r: 'tous les chiffres Hebraïques ont double sens, l'un appert et l'autre caché'; f. 38r (Adam naming creation); f. 41r: 'il est expressement dict, que le monde fut fabriqué par les 22 lettres de l'alphabet'; f. 42r: (rapport des lettres de l'alphabet Hebraïque, aux choses créees) 'Et ont este les Cabalistes si speculatifs, peraventure trop curieux, entant que la coniecture de l'esprit humain s'est peu estendue, de penser par les divers assemblemens des lettres, atteindre à sçavoir le nombre des choses créees … Car de la diversité des Ziruphs, ou accouplemens, et suittes de lettres, sans aucun meslange de points, vient resulter un nombre, qui est autant comme infini pour nostre regard: assavoir 11240025908271968000.

ideas, as Frances Yates notes; they are also detectable in the works of writers such as John Dee and Robert Fludd. There is moreover a connection to biblical chronology (in which Harriot appears to dabble) and even to millenarianism, and to the belief in the power of incantations invoked in sympathetic magic and even medical cures.[19]

Anagrammatology is a somewhat different affair. The best-known treatise on it, which traces its history back to ancient times and sets out its practices, is that of Guillaume Le Blanc (1551–1601), the Bishop of Toulon, which appeared in Rome in 1586 and in Frankfurt in 1602.[20] It is linked in its origins to Hebrew, Greek, and Christian number and letter manipulations, and its popularity throughout Europe in the late sixteenth century is attributed to the Cratylic enterprise of the Parisian-trained poets. However, in fact, it does not make much of their claim to release mystical properties contained in words and to reveal higher orders of knowledge about the universe. A prosaic definition is given (an anagram is 'a short clause which is made up of the artful transposition of all (not just some) of the letters of a given name');[21] the practice is extended to chrono-anagrams (ones by which numbers or dates are extracted from a text by giving values to the Roman letter numerals M, D, C, X, V and I); and it is related to the rhetorical practice of etymology (that is, the attribution of meanings to parts of words which account for their definition, e.g. 'testament' as 'testis mentis').[22] Its practice is a great deal laxer than its definition suggests, for Le Blanc allows anagrammatologists to leave out letters when it suits them, to repeat letters for their own convenience, to add letters, to exploit the ambiguity in Renaissance printing between 'i' and 'j', 'u 'and 'v', to use alternative spellings of proper names (Guglielmus: Guliermus)

Que si l'on y veut adiouster les points, le nombre ne se pourroit pas exprimer, ny concevoir presque de nous.' See also J. Dan (ed.), *The Christian Kabbalah* (Cambridge, MA, 1997). The sum of combinations of the alphabet is found also in the medieval period: see BL MS Sloane 2156, f. 128v (Henry of Hesse), cited by M. Clagett (ed.), *Oresme and the medieval geometry of qualities and motions* (Madison, Milwaukee and London, 1968), p. 447.

[19] F. Yates. *The occult philosophy in the Elizabethan age* (London, 1979); see also J. Dan, 'The kabbalah of Johannes Reuchlin and its historical significance', in Dan (ed.), *The Christian Kabbalah*, pp. 55–96; BL Add. MS 6789, f. 471v; Maclean, *Logic, signs and nature in the Renaissance*, p. 111–12.

[20] *Libellus de ratione anagrammatismi* (Rome, 1586); N. Reusnerus, *Anagrammatographia ... accessit Gulielmi Blanci Libellus de ratione anagrammatismi*, ed. E. Reusnerus (Jena, 1602). See also Gisèle Mathieu-Castellani, 'Nombre, lettre, figure à la Renaissance', *Revue des sciences humaines*, 179 (1980), 5–6; and Paul Zumthor, 'D'une pensée littérale', *Revue des sciences humaines*, 179 (1980), 7–21.

[21] Le Blanc, *Libellus*, in Reusnerus, *Anagrammatographia*, sig. A7v: 'clausula, quae ex artificiosa literarum omnium, neque plurium alicuius nominis transpositione componitur'.

[22] On this practice, see I. Maclean, *Interpretation and meaning in the Renaissance. The case of law* (Cambridge, 1992), pp. 109–10. See also Le Blanc, *Libellus*, in Reusnerus, *Anagrammatographia*, sig. C4v (which refers both to Cratylism and etymology in this sense).

and to mix languages (to derive French from Latin, or vice versa, for example). It hardly surprising after this catalogue of laxities that he can find only one fully satisfactory example: CUIAS (the distinguished French Jurist Jacques de Cujas) and CAIUS (the Roman jurisprudential writer), and even this relies on substituting an ‘i’ for a ‘j’.[23] Anagrams were used as a learned humanist game, as means of flattery or celebration and as satirical weapons. They sometimes implied that they released hidden significance from the rearrangement of letters, but this is not a programmatic claim.

They were certainly popular. Nicolaus Reusnerus’s *Anagrammatographia* of 1602 contains 682 pages of them in nine books; a more humble collection closer to Harriot’s home is that of WiIliam Cheke, *Anagrammata, et chron-anagrammata regia* of 1613, written as a consolatory document for James I after the death of Prince Henry Stuart in 1612, involving Latin and Greek, and chronological anagrams (and combinations of these with letter anagrams) of some complexity. At the same time in France, a certain Thomas Billon obtained royal favour from Louis XIII for a similarly sedulous (and according to him strictly accurate) book of anagrams and was encouraged to continue producing them for the edification and amusement of the French court.[24] The appropriateness of the anagram to the character (in Cheke, HENRICUS SEPTIMUS becomes PIUS ITEM SINCERUS and MARIA REGINA becomes EI ARMA NIGRA) is less striking than the aim to show ingenuity, to flatter or to express opprobrium.[25]

It is instructive to compare what Harriot does with the practice of other mathematicians. Michael Stifel, whose books on arithmetic Harriot cited and sometimes refuted, was a near-contemporary and follower of Luther who attempted to link the mystical force in names to the practice of anagrams in the work he entitled *A very remarkable word-reckoning together with a noteworthy explanation of some numbers in the book of Daniel and the book of Revelations*, which appeared in Königsberg in 1553. There he sets himself the task of showing that Pope Leo X was the Antichrist (whose number, according to Revelations,

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23 Le Blanc, *Libellus*, in Reusnerus, *Anagrammatographia*, sig. C8r. See also A4–5, where cabbalistic practices are mentioned, and B2r–v, where there is a reference to Daurat and Ronsard.

24 *Le bon ange de la France, rapportant soixante-deux anagrammes en forme de presages, voeux et benedictions, le tout heureusement tiré sans addition, diminution ou mutation de lettres du tres-fortuné, tres-grand et tres-auguste nom de Louis XIII de Bourbon, roi de France et de Navarre; ensemble de tres-haute et tres-illustre princesse Anne d’Austrie, infante d’Espagne, sur l’heureux mariage de Leurs Majestés* (Lyon and Dijon, 1613); *Sibylla gallica, anagrammaticis magna praedicens oraculis, idque duabus et ultra centuriis, stylo partim soluto, partim versificato, nulla mutata, dempta vel addita littera, in gratiam christianissmi principis Lodovici XIII Galliae et Navarrae regis felicissimi, necnon Annae Mauritiae de Austria, reginae* (Paris, 1616; 2nd edn, 1624). The claim that he obtained royal favour is found in M. Curl, *The anagram book* (London, 1982), p. 11.

25 W. Cheke, *Anagrammata, et chron-anagrammata regia* (London, 1613), sig. E1v, E2v.

is 666); he wrote Leo DeCIMVs and derived the chronogram MDCLVI 1656 from the name (but it could equally be 1654). He then had revealed to him in a divinely inspired dream that M stood for 'Mysterium', not 1,000, and that he was allowed to add in 'X', this having 'tenth' represented both as a word and a letter; from this he reached the desired total: 666.[26] Girolamo Cardano, another mathematician cited by Harriot, also writes about word patterns (such as those of Bishop Rhabanus Maurus), hidden meanings, Sybilline utterances and mystical associations of words in his popularizing works the *De subtilitate* of 1550 and the *De rerum varietate* of 1557, although, to be fair to him, it should be pointed out that in these contexts at least he pours scorn on them.[27]

What of Harriot himself? He shows himself also to be interested in the shapes of numbers: at one point, he sets out 1–9 all written in straight lines only, prefiguring the practice of digital screens.[28] But this can hardly be said of itself to reflect an occult cast of mind. He certainly rose to the challenge set by Galileo to Johannes Kepler in 1610 of an anagram hiding an astronomical message: s/mais/mrm/il/m/epoe/taleum/ibon/enugttauias (the answer being 'altissimum planetam tergeminum observavi': 'I have observed the most distant planet as thrice-begotten'). Harriot knew of the challenge from Kepler's publication in answer to it published in 1610[29] and must have been dissatisfied with Kepler's attempt to solve it ('salve umbistineum geminatum Martia prolis': 'greetings o lumpy son of Mars'); as such, he set about trying to solve it himself. I have found about 10 attempts or partial attempts to solve the puzzle in Harriot's papers; John North reports that there are over 50. There is some evidence that Harriot tried to adopt a methodical approach to the solution of Galileo's anagram (using techniques such as letter distribution), affording evidence of his analytical mind.[30] This is a clear example of a parlour game, not of mystical meanings being attributed to letters.

Harriot also engages in a number of anagrammatic transformations of his own name (sticking closer to the rules than many others); some of these may have been

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[26] *Ein sehr wunderbarliche wortrechnung sampt einer merckliche erklerung etlicher zalen Danielis und der Offenbarung Sanct Johannis* (Königsberg, 1553), sig. A3r. See also his earlier *Ein Rechen Büchlin. Vom EndChrist* (Wittenberg, 1532). For reproductions of some of Stifel's mathematical texts referred to here, see M. Folkerts, E. Knobloch and K. Reich (eds), *Mass, Zahl und Gewicht. Mathematik als Schlüssel zu Weltverständnis und Weltbeherrschung* (Wiesbaden, 2001), pp. 66–89.

[27] See *De subtilitate*, xv ('De inutilibus subtilitatibus'), in *Opera omnia*, ed. C. Spon, vol. 3 (Lyon, 1663), pp. 589–90; *De rerum varietate*, x.51, ibid, iii. 207–8; see also below, note 48.

[28] BL Add. MS 6789, f. 30v.

[29] *Dissertatio cum nuncio sidereo nuper apud mortales misso a Galilaeo Galilaeo* (Prague, 1610).

[30] See J.D. North, 'Thomas Harriot and the first telescopic observations of sunspots', in J.W. Shirley (ed.), *Thomas Harriot. Renaissance scientist* (Oxford, 1974), pp. 137–8; BL Add. MS 6786, ff. 251v, 303r.

thought up as a witty response to the puzzle set to Johannes Kepler by Galileo: 'oho trahit musas'; 'oho trahis mutas'; 'oho sum charitas'; 'tu homo artis has'; 'homo hus ut artis'; 'homo hasta utris/vitus/vutis'; 'humo astra hosti'; 'trahe hosti musa'; 'a trahit has musa'; 'oh, os trahit musa' (all from 'Thomas Hariotus').[31] These look to me not to be claims about revelation of character (though 'oho sum charitas' must have pleased him) but more like the product of a few idle moments spent by someone who, if he lived today, would have revelled in crosswords. From what I have seen of his manuscripts, Harriot, unlike Stifel or Cardano, seems to betray no inclination to see mystical or occult senses in letter combinations.[32]

Atoms

Much has been written about the various accusations of impiety, atheism and atomism directed at Harriot or figures such as Ralegh and Marlowe with whom he was associated.

I do not need here to repeat this material.[33] Nor do I need to set out the various versions of atomism current at this time: the work of John Henry and John North, and the volume edited by Christoph Lüthy and others have done this task thoroughly.[34] A popular source for this doctrine was Lucretius's *De rerum natura* (ii.61ff.), which speaks of collocations and combinations of atoms (which are irreducible and vary in size and density) in an infinite or indeterminate universe, in which the vacua between atoms is presupposed; Aristotle's various accounts of Democritean natural philosophy were also accessible and were seen by some, including Francis Bacon in the *Advancement of learning* of 1605, as a valid alternative to Aristotelian physics:

> The Natural Philosophie of *Democritus*, and some others who did not suppose a *Minde* or *Reason* in the frame of things, but attributed *the form thereof able*

[31] BL Add. MS 6789, f. 475v.

[32] It is pertinent here to note that Harriot himself devised his own phonetic alphabet when in the New World. See Shirley, *Thomas Harriot*, op. cit. (note 14), pp. 108–11.

[33] See Shirley, *Thomas Harriot. A biography*, pp. 198ff; J.J. Roche, 'Harriot, Oxford, and twentieth-century historiography', pp. 229–45 and Mandelbrote, 'The religion of Thomas Harriot', pp. 246–79, both in Fox (ed.), *Thomas Harriot*; S. Pumfrey, 'Was Thomas Harriot the English Galileo? An answer from patronage studies', *Bulletin of the Society of Renaissance Studies*, 21 (2003), at 21, suggests that 'Harriot was inhibited from writing or publishing even on the vacuum, let alone atomism, the motion of the Earth or the corruptibility of heavenly bodies' because of a lack of a powerful patron to protect him.

[34] J. Henry, 'Thomas Harriot and atomism: a reappraisal', *History of science*, 20 (1982), 267–96; J. North, 'Stars and atoms', in Fox (ed.), *Thomas Harriot*, pp. 186–228; C.H. Lüthy, J.E. Murdoch and W.R. Newman (eds), *Medieval and early modern corpuscular matter theories* (Leiden, 2001).

> *to maintain it self to infinite essaies or proofes of Nature*, which they tearme *fortune*: seemeth to mee (as farre as I can iudge by the recitall and fragments which remain vnto vs) in particularities of Phisicall causes more real and better enquired then that of Aristotle and Plato.[35]

Bacon sets this comment in the context of his map of disciplines, to which I have already alluded: what he calls natural science or 'phisick' is dedicated to the enquiry into 'variable or respective' (i.e. material and efficient) causes, while 'metaphysick' (in the sense I have already given of the collection of axioms which are presupposed in any enquiry into nature) enquires into fixed and constant (i.e. final and formal) causes. Bacon records the view that these may lie outside the reach of man, but thinks them worthy of pursuit provided that their study can further man's capacity to manipulate nature. The particular deficiency he finds in metaphysick lies in its pursuit of final causes (teleology), which he identifies as a feature not only of Platonic but also of Aristotelian and Galenic philosophy:

> For to say that the haires of the Eyeliddes are for a quic-sette and fence about the sight: Or, That the firmness of the Skinnes and Hides of liuing creatures is to defend them from the extremities of heate and cold: Or, that the bones are for the columnes or beames, whereupon the Frames of the bodies of liuing creatures are built; … and the like, is well inquired and collected in METAPHYSICKE, but in PHYSICKE they are impertinent.[36]

Bacon's recommendation of the study of only material and efficient causes, and his exemplary translation of the etiology of the eyelash into a form which does not rely on the determination of purpose ('*Pilsotie is incident to Orifices of Moisture*') seems to me, by setting aside the study of teleology, to be the most radical critique of Aristotelian zoology of its day and to mark a watershed in the recommended procedures of natural enquiry. This cannot proceed without presuppositions or axioms, but for Bacon these are provisional and revisable; the natural historian uses the keenness of his perception ('sagacitas') and his orderly approach to experiment to produce not a general theory with laws but no more than 'interim regularities'.[37]

I shall return to the theological implications of this below; here I am interested in Bacon's collocation of this with his rejection of Aristotle and praise of

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35 Bacon, *The advancement of learning*, p. 86.

36 Ibid., p. 86. The examples appear to be taken from *De partibus animalium*, 653 a 30; 654 a 30ff.; 658 b 1ff.) and Galen, *De administrationibus anatomicis*, I.2, K 2. 220, 226 (on bones). It is more likely that Bacon found the Galen reference through an intermediary, such as A. Vesalius, *De humani corporis fabrica* (Basle, 1543), i.1, p. 1.

37 See L. Jardine, '*Experientia literata* or *Novum Organum?* Bacon's two scientific methods', in W.A. Sessions (ed.), *Francis Bacon's legacy of texts* (New York, 1990), pp. 47–67.

Democritus, and his awareness that atomism leads to a physics of chance. Another feature of this rejection is the new importance placed on accidental features of phenomena in Aristotelian terms, which are dismissed from consideration in an essentialist natural philosophy driven by the pursuit of final causes and committed to a peripatetic account of matter.[38]

That Harriot was committed to an investigation of atomism is known through his letters to Kepler and through his pupil Nathaniel Torporley's refutation of his position, from which it is deduced that Harriot defends the eternity of the matter of the world and the doctrine 'ex nihilo nihil fit', both doctrines being taken to be inconsistent with the Judaeo-Christian account of creation.[39] In fact, 'ex nihilo nihil fit' is not just Lucretian; it is also Aristotelian, as orthodox exponents of that doctrine concede (it is the first of the Aristotelian Philip Melanchthon's nine 'axioms of nature').[40] Harriot records the tag on at least one occasion in his papers[41] and links it to the perfectly unexceptionable doctrine of the 'impossibilitas penetrationis dimensionum' (two bodies cannot occupy the same place at the same time). However, the other two impossibilities of Aristotelian physics – the 'impossibilitas infiniti et vacui' – are attacked (the first at length) and there are other very unorthodox positions recorded in Harriot's manuscripts Stephen Clucas has recently shown that he was interested in the doctrine derived from the medieval Islamic philosopher Al Kindi of 'vis radiativa' (by which everything produces rays whether substance or accident, and the universe is a vast network of forces in which every creature is a source of radiation).[42] This leads Harriot to refer to the quality of whiteness as 'accidental form', a scholastic term used in

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38 See I. Maclean, 'White crows, greying hair and eyelashes: problems for natural historians in the reception of Aristotle's logic and biology from Pomponazzi to Bacon', in G. Pomata and N. Siraisi (eds), *Historia. Empiricism and erudition in early modern Europe* (Cambridge, MA and London, 2005), pp. 147–79.

39 'A synopsis of the controversie of Atoms', published as an appendix to J. Jacquot, 'Thomas Harriot's reputation for impiety', *Notes and records of the Royal Society of London*, 9 (1952), 164–87. See also J.F. Wippel, 'The condemnations of 1270 and 1277 at Paris', *Journal of medieval and renaissance studies*, 7 (1977), 169–201.

40 See 'axiomata physica', in MS Vatican Pal. Lat. 1038, fol. 2 ('Physicae seu naturalis philosophiae compendium'): '1. Ex nihilo nihil fit 2 In omni generatione oportet subiectum esse cognatum, quod recipit Formam 3 Contraria a contrariis corrumpuntur 4 Similia a similibus oriuntur et aliuntur 5 Generatio unius est corruptio alius 6 Nulla magnitudo est infinita 7 Nullum est actu infinitum 8 Natura determinata est ad unum 9 Omnis motus fit in tempore'; see also Aristotle, *Physics*, i.4,187a 27–9; Lucretius, *De rerum natura*, i.1,206.

41 BL Add. MS 6788, f. 493r, cited by Gatti, 'The natural philosophy of Thomas Harriot', p. 71 and Mandelbrote, 'The religion of Thomas Harriot', pp. 258–9, both in Fox (ed.), *Thomas Harriot*.

42 BL Add. MS, 6786, f. 428r; MS 6789, f. 572r; S. Clucas, 'Corpuscular matter theory and the Northumberland circle', in Lüthy, Murdoch and Newman (eds), *Medieval and early modern corpuscular matter theories*, pp. 181–201. The three impossibilities of Aristotelian physics are listed on the title page of MS Vatican Pal. Lat. 1038 (see above, note 39).

contradistinction to the more commonly encountered 'substantial form', referring to an actual (not potential) accidental feature (of which 'whiteness' is the standard example) which cannot exist separately from the subject in which it inheres.[43] The distinction between accident and essence sits uneasily in its scholastic context, threatening the coherence of definitions in natural philosophy by excluding features of them (e.g. the blackness of a crow) and making certain sorts of taxonomic claims sound very odd, but it is much less out of harmony with atomistic (and alchemical) philosophy,[44] as can be sensed in the letter that Harriot's patron, the Earl of Northumberland, wrote to his son about alchemical theory in 1594:

> The doctrine of generation and corruption unfoldeth to our understanding the method general of all attomycall combination possible in homogeneall substances, together with the ways possible of generating the same substance as by semination, vegetation, putrefaction, congelation, concoction etc with all the accidents and qualities rising from these generated substances, in hardnes, softnes, hevines, lightnes, tenacitie, frangibilitie, fusibilitie, ductibilite, sound, coulor, tast, smell, etc.[45]

The link between the size, shape, and configuration of atoms and accidental features of matter such as weight, homogeneity, solidity and brittleness is here made to sound more coherent than in the Aristotelian account.

There is no very clear message which emerges from the scattered references to fundamental physics in Harriot; however, I would venture to suggest that he, like Bacon, sets aside a teleological view of nature and tries to limit his investigations to circumscribed areas of enquiry. One document which as far as I know has not attracted much attention is the following list of the presuppositions of Aristotelian natural philosophy (recorded here sequentially to save space) which Harriot at one point felt moved to draw up:

> 1 principium 2 causa 3 elementa 4 natura 5 necessarium 6 unum multa 7 ens 8 essentia 9 idem/differentia/simile/dissimile 10 opposita 11 prius, et posterius 12 vis facultas potentia/imbecillitas seu impotentia posse, seu possibile/non posse, seu impossibile 13 quantum seu quantitas 14 qualitas 15 Re[so]luta? 16 perfectum 17 extrema, seu terminus 18 kata ti, seu id quod per se 19 dispositio Habitus 20 affectio seu passio 21 privatio 22 habere (aliquid in alio) 23 unum ex alio 24 pars 25 totum 26 mancum seu imminutum seu mutilum 27 genus differen[tia] generis 28 falsum 29 accidens

43 Aquinas, *De ente et essentia*, vi. 1.

44 See Maclean, 'White crows, greying hair and eyelashes'; in 'Corpuscular matter theory and the Northumberland circle', Clucas points out that everything (whether substance or accident) produces rays; the distinction therefore no longer causes taxonomic difficulties.

45 Cited by Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', p. 108.

<table>
<tr><td>Tempus</td><td>| est, non est</td><td></td></tr>
<tr><td>Spatium</td><td>| finitum, infinitum</td><td></td></tr>
<tr><td>vis</td><td>| potentia, actus</td><td>esse posse</td></tr>
<tr><td>materia</td><td>| simplex compositum</td><td>mi[x]tio? element[orum]</td></tr>
<tr><td></td><td>| universale particulare</td><td></td></tr>
<tr><td></td><td>| idem diversum</td><td>simile dissimile</td></tr>
<tr><td></td><td>| per se, per aliud</td><td>subiectum, accidens</td></tr>
<tr><td></td><td>| reale, rationis</td><td></td></tr>
<tr><td>determinatum</td><td colspan="2">| absolutum, aut aliud totum pars perfectum imperfectum</td></tr>
<tr><td>indeterminatum</td><td colspan="2">| mutabile, immutabile bonum, malum, indaiphoron</td></tr>
<tr><td></td><td>Aeterna, temporale</td><td></td></tr>
<tr><td>coitus [permeandi?]</td><td>In fluxu</td><td>in facto, in fide</td></tr>
<tr><td>discretio</td><td>unum plura</td><td>pauca multi</td></tr>
<tr><td></td><td></td><td>magnum parvum[46]</td></tr>
</table>

The composition of this list is in itself worthy of detailed study, for which there is no space here: it is an amalgam of presuppositions and premises drawn from parts of the Aristotelian Organon (notably the *Categories*) with relevant passages from the *Physics* and the *Metaphysics*. I do not believe Harriot set this down in the spirit of a Baconian metaphysics, but rather as a way of surveying critically the extensive metaphysical baggage which Aristotelian physics carries with it. To set out all the presuppositions of an argument was enjoined upon disputants in traditional universities, and although it was condemned as a practice in the medieval period for being potentially subversive to the doctrine of the unicity of truth, it is a frequently encountered practice in textbooks of natural philosophy and is a feature of the 'mos geometricus'.[47] Harriot's version of this exercise is perhaps more perspicacious than many.

Numbers

I come finally to my third area of enquiry. The mathematics of the Renaissance is sometimes characterized as transitional, lying between the 'passive, mystical and irrational approach' of the Middle Ages and the 'active rational problem-solving' which characterizes the new science of the seventeenth century; according to this

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[46] BL Add. MS 6789, f. 511r–v. Compare the remark by J. Jacquot, 'Harriot, Hill, Warner and the new philosophy', in Shirley (ed.), *Thomas Harriot. Renaissance scientist*, p. 125: '[Harriot] did not rely on metaphysical speculation but on evidence.'

[47] The proposition 'quod nichil est credendum, nisi per se notum, vel ex per se notis possit declarari' was condemned by Bishop Tempier in 1277. See E. Serene, 'Demonstrative science', in N. Kretzmann, A. Kenny and J. Pinborg (eds), *The Cambridge history of later medieval philosophy* (Cambridge, 1982), p. 507; and Maclean, *Logic, signs and nature in the Renaissance*, pp. 114–18.

rather Whiggish account, the sixteenth century continues to study mathematics for what it reveals about the cosmos, the soul and the divine, and it pursues enquiries into the properties of numbers in a Pythagorean spirit.[48] Cardano gives coherent expression to this approach:

> [By arithmetic] we are taught that everything is bound together by a certain marvellous and hidden order. Nor is it to be believed that this connection is fortuitous, but rather the shadow of a divine bond, through which everything is mutually tied together in a certain order, measure and time: for this reason the Pythagoreans and Academics not altogether randomly constituted numbers as the origins of things: and who would doubt, if Aristotle had not egregiously misunderstood them on this matter, that they meant anything else than that numbers are the shadows of that order by which God constituted, made and ordered everything? … The order [of arithmetic] … is infinite in itself [per se], and nothing other that the shadow or trace of the infinite order … Our mind, by contemplating it, sees the godhead as if through a tiny chink.[49]

These sentiments are also found at this time in France and England, in works by Jacques Lefèvre d'Etaples, Josse Clichtove, Girard Roussel and Jacques Peletier du Mans on the one hand, and John Dee's 'Mathematical Praeface' to the translation of Euclid's *Elements* by Sir Henry Billingsley of 1570 on the other.[50]

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[48] A.E. Moyer, 'The demise of the quadrivium and the beginning of the scientific revolution: Boethius in the sixteenth century', *Intellectual news*, 10 (2002), 69–77.

[49] *De libris propriis*, in *Opera*, i.143: 'arithmeticae contemplatio subtilissima est: et per se felicissima, tum quia docemur cuncta esse miro quodam ordine et arcano connexa. Neque enim credendum est illam connexionem esse fortuitam, sed umbram quandam vinculi divini, quo cuncta invicem colligata sunt certo ordine, mensura, tempore: ob id non tam temere Pythagorici et Academici numeros constituerunt rerum principia: quos si non per calumniam Aristoteles voluisset interpretari, quis dubitat non aliud per hoc aenigma illos significasse, quam numeros umbras esse eius ordinis quo Deus cuncta constituit, fecit et ordinavit? Sed ordo ille in Deo quasi involutus est, et unum quoddam, extra autem multiplex: qui cum sit infinitus undequaque, quis non videt infinitam primi boni esse naturam? Nam neque numerus per se est, est enim accidens: neque sui autor: Deus enim non est: nec αὐτοποιός, neque animae nostrae figmentum, sic enim falsus esset: natura igitur quaedam per se infinita est, nec aliud quam umbra aut vestigium ordinis infiniti. Ecce quam parvo deprehendimus infinitam esse naturam. In illum igitur intuens animus noster, divinitatem quasi e rimula inspicit.' See also Aristotle, *Metaphysics*, i. 5, 985 b 20 (on Pythagoras); i. 9, 990 a 30 (on Plato).

[50] See, for example, I. Pantin 'La representation des mathématiques chez Jacques Peletier du Mans: cosmos hiéroglyphique ou ordre rhétorique?', *Rhetorica*, 20 (2002), 375–90 (383–9); and Dee quoted by A. Marr, 'Curious and useful buildings: the mathematical model of Sir Clement Edmondes', *Bodleian Library record*, 18 (2003), 108–50 (127): 'and for us Christenmen, a thousand mo occasions are, to have need of the help of Megethodicall Contemplations: whereby, to trayne our Imaginations and Myndes, little by little, to forsake

According to these writers, the patterns and correspondences of mathematics reveal the beauty of the order of the cosmos, and one of the ways through which these are to be discovered is the investigation of numbers, including the properties of their combinations and permutations.

The combinations I wish to look at here are found in the prehistory of what we now know as Pascal's triangle, some of whose properties have been known since ancient times. For its history, I shall be relying mainly on the excellent recent monograph by A.W.F. Edwards, who distinguishes three contexts in which a triangle which generates similar numbers and properties arises: figurate or polygonal numbers; combinatorial numbers; and binomial coefficients.[51] The first of these are generated from the practice of arranging dots in shapes such as triangles or squares, or three-dimensional figures such as triangular pyramids or tetrahedra. From the progression of numbers, tables can be drawn up such as the one to be found in Stifel's *Arithmetica integra* of 1544.[52] Stifel uses this in connection with the extraction of roots, and hence links it to binomial coefficients, which are obtained from the expansion of $(1 + x)^n$. The table of combinations has the same progression of numbers, the combination being given by ${}^nC_r = n!/r!\,(n\text{-}r)!$ where c is the number of selections that can be made, r at a time, from n objects, regardless of any different arrangements which can then be made (thus the permutations of abc – acb, bca, bac, cab, cba – count as one selection only). Cardano reproduces the figurate table and the table of combinations in one of his last works, the *Opus proportionum* of 1570, in which he also notes the relation of his table of combinations to the geometrical progression 2^n (for $n>2$, $2^n - 1 - n$ gives the total number of combinations of n) and seeks to enunciate a rule which would obviate the need to write out the whole triangle to discover binomial expansions and sums of combinations (some of this material had already appeared in his *Practica arithmetice* of 1539).[53]

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and abandon, the grosse and corruptible Objects of the outward senses: and to apprehend, by sure doctrine demonstrative, Things Mathematicall. And by them readily to be holpen and conducted to conceive, discourse, and conclude of things intellectual, spirituall, aeternall, and such as concerne our Blisse everlasting.'

51 A.W.F. Edwards, *Pascal's arithmetical triangle* (London and New York, 1987). On figurate numbers, see also J.E. Murdoch, *Album of science. Antiquity and the Middle Ages* (New York, 1984), p. 99.

52 *Arithmetica integra* (Nuremberg, 1544), f. 44v, illustrated in Folkerts, Knobloch and Reich (eds), *Mass, Zahl und Gewicht*, p. 81.

53 *Practica arithmetice*, li, *Opera*, iv.73; Cardano, *Opus novum de proportionibus*, *Opera*, iv. 557–8. The rule (quoted by Harriot, BL Add. MS 6782 f.44r) reads: 'ut autem habeas numerum singulorum ordinum, in quavis multitudine, deducito numerum ordinis a primo, et divide per numerum ordinis ipsius reliquum, et illud quod provenit, ducito in numerum maximum praecedentis ordinis, et habebis numerum quaesitum'. Cardano felt obliged to give an example of this working: 'velut si sint undecim, volo scire breviter numeros, qui fiunt ex variatione trium.primo deduco pro secundo ordine 1 ex 11 fit 10. divido per 2. numerum ordinis, exit 5. duco in 11 fit 55. numerus secundi ordinis. Inde

These passages are transcribed by Harriot in his worksheets headed *Of combinations* and the treatise entitled *De numeris triangularibus et inde de progressionibus arithmeticis*, which Edwards dates to 1611 or earlier.[54] Harriot seems to have contemplated the publication of a work on triangular numbers, because at one point he sketches out its titlepage.[55] In it, he draws on all the accumulated work of the sixteenth century and adds to it a statement of some axioms which are not found in the work of his predecessors, linking his work on combinations with his interest in infinites.[56] Moreover, he demonstrates a clear consideration of negative and fractional values in respect of combinations and what has come to be known as the Newton-Gregory forward-difference formula, as well as dealing with permutations and binomial coefficients.[57] He aspires to find a 'generall rule to get the mayne summe of all the complications of any number of species' and 'a general method for the particular summe without the table of combinations or complications'. As in the case of Cardano, this falls short of purely symbolic notation (an innovative feature of Harriot's work on algebra), as it relies on verbal formulae, worked examples and the location of numbers in a table.[58]

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detraho 2. qui est numerus differentiae ordinis tertii a primo ex 11. relinquitur 9. divido 9 per 3 numerum ordinis exit 3. duco 3 in 55. numerum secundi fit 165 numerus tertii ordinis'.

[54] BL Add. MS 6783, ff. 403ff.; Edwards, *Pascal's arithmetical triangle*, p. 11.

[55] BL Add. MS 6782, f. 146v: 'THOMAE HARIOTI/Magisteria/Numerorum Triangularium/Et Inde/Progressionum Arithmeticarum'; he is not alone among late Renaissance scientists to engage in this innocent act of vanity: see also Milan Bibliotheca Ambrosiana D 235 Inf, ff. 13, 15 (two mock-ups of titlepages of unpublished works by their aspiring author Guiseppe Moletti). I am grateful to Roy Laird for this reference.

[56] E.g. BL Add. MS 6786, f. 363r: 'And yet for an ease in describing progressions we must … understand a quantity absolutely indivisible but multiplicable infinitelie … till a quantity absolutely unmultiplicable be produced which I may call universally infinite.' For Harriot's consideration of infinites, see BL Add. MS 6782, ff. 199, 362–74: MS 6784, ff. 359–64, 428–9; MS 6785, ff. 190–91, 436–7.

[57] BL Add. MS 6782, ff. 29, 46, 74v, 96ff., 147r (figurate numbers); 30ff. (Pascal's triangle), 108–9 (binomial coefficients), 145 (negative numbers and fractions), 150 (an algorithm for figurate numbers), 180ff. and 331ff. (tables of combinations); BL Add. MS 6783, ff. 403ff. ('De numeris triangularibus et inde de progressionibus arithmeticis').

[58] BL Add. MS 6782, f. 38r: 'according to the number of species, understand as many termes to be given in continuall proportion, or progression, beginning at the [unit] and making everie terme double to his precedent: the double of the last term less [a] unit is the summ desired … As for example I would knowe all the complications of 6 species together with the number of the simples the sixth terme of such a progression I spake of is 32. The double less [a] [unit] is 63, the summ of all the complications with the number of simples which were sought. Of the number of species be greate the last term desired is to be gotten by the rule of progression in arithmetick. The reason of the rule is easilie to be contrived out of the particular constructions in another paper annexed.' There is another formulation derived from Cardano on f. 35v: 'by this manner of construction and generation of the variety of combinations or complications [a set of tables setting out combinations of letters from a to g] these propositions are manifest: The number of complications with the number

I refer the reader to Edwards's very positive judgement and clear exposition of Harriot's achievement; I intend here to measure it in another way by comparing what Harriot writes with the work of other near-contemporary mathematicians. I shall offer one or two very brief examples: Stifel uses the figurate numbers in his millenarian work *A very remarkable word-reckoning* to which I have already referred. There he makes letters correspond with figurate numbers; for the purposes of his apocalyptic chronology, he needs to get to the figure 5,200 (the sum of two alphabets of figurate numbers) which is the sum of two mystical numbers taken from book of Daniel (1,290 + 1,335) and two taken from the book of Revelations (666 (number of the beast) + 1,260). The sum very nearly works (it is short by 49); to make this up, he chooses to add in the sums of two words ecce (42) + ac (7), in a way which would have been grudgingly allowed by the lax anagrammatologist Guillaume Le Blanc but by few others.[59] The calculation is linked to biblical chronology, in which Harriot also dabbled: as Stephen Clucas says, it is an activity 'which do[es] not sit well with a progressive scientific narrative'.[60] It is not clear to me, however, that Harriot is doing this on his own behalf; it may very well have been an enquiry commissioned by a colleague or patron.

Other writers link combinations to practical concerns: Luca Pacioli (c. 1445–1514) to table placements; Cardano and Niccolò Tartaglia (1499–1557) to gambling; the medical professor Sanctorius Sanctorius to diagnosis (in his *Methods of avoiding error* of 1603, he wanted to compute the combinations of 3 and 4 malignant humours out of the possible 165 such humours in a human body in order to show how many combinations had to be considered by the rational doctor, and got the sum horribly wrong, by extrapolating the rule of two combinations [$\frac{1}{2}n(n-1)$] to combinations of three and four in the form $\frac{1}{2}(n-1)(n-2)$ and $\frac{1}{2}(n-2)(n-3)$.[61] It is interesting to note, in respect of practical concerns, that the first printing of Pascal's triangle was on the titlepage of a book of arithmetic produced for use in

of their simples is double to the number of complications with their simples of the next precedent order [and] one more. In any order of complications the number of bynaries (ternaries, quaternaries, &) is equal to the number in the precedent order of binaries and simples (ternaries and binaries, quaternaries and ternaries, &c).' The rule $2^n - 1 - n$ is shown in a table but not set down in symbols. See also Harleyan MS, 6002, ff. 10v–13v for a clearer transcription of some of this material.

59 Stifel, *Ein sehr wunderbarliche wortrechnung*, sigs. C4–D1; see also R. Barnes, *Prophecy and gnosis. Apocalypticism in the wake of the Lutheran Reformation* (Stanford, CA, 1988), pp. 188ff.

60 Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', p. 103.

61 On Pacioli, Tartaglia and Cardano, see Edwards, *Pascal's arithmetical triangle*, pp. 37ff.; Sanctorius Sanctorius, *Methodi vitandorum errorum* (Geneva, 1630), vii.9, pp. 630–39; and Maclean, *Logic, signs and nature in the Renaissance*, p. 176.

commerce (Peter Apian's *A new and sound book of instruction in accounting for merchants* of 1527).[62]

Harriot's manuscript treatise is not only given exclusively in his own notation, as opposed to discursive Latin; it also has no suggestion as to any practical application. The pure speculation about numbers and their properties is found elsewhere in the manuscripts.[63] There is never any hint that the patterns are thought to be of mystical origin, or to offer paths to understanding the divine mind, or proofs of the immortality of the human soul;[64] this is also true, it seems to me, of Harriot's various enquiries into mathematical infinity.[65]

Harriot: believer, natural philosopher, mathematician

It does not seem to me that Harriot, for all his interest in matter and 'vis radiativa', was beguiled by hermetic or occult thought; he appears to me to have conducted his investigations into words, atoms and numbers in a dispassionate and highly abstract spirit, without direct reference to the utility or religious significance of any discoveries which might be made in these areas of research. Equally, although his work on alchemy or on practical issues such as longitude may be characteristic of the 'mathematical sciences' in that they combine theory with strongly practical concerns, this does not seem to be true of much of the work recorded in the voluminous manuscripts which have come down to us. These reflections bear on the question of Harriot as a 'devout worshipper of the triune God'. There are a number of tantalizing clues which might be picked up here, from Harriot's account of the religion of the Amerindians of Virginia (in which he includes a reference to the political justification for a doctrine of the after-life, a justification much decried by theologians)[66] to enigmatic asides in the MSS which have exercised a number of Harriot scholars. Harriot's pupil Nathaniel Torporley produced a work with the title *Corrector analyticus*, whose titlepage announces that as an outstanding

[62] *Eyn New Unnd wolgegründte underweysung aller Kauffmannß Rechnung* (Ingolstadt, 1527). See also K. Reich, in Folkerts, Knobloch and Reich (eds), p. 81.

[63] E.g. BL Add. MS 6782, ff. 27–8 (on magic letter squares), 29, 46, 74v, 96ff. and 147r (figurate numbers), 30ff. (Pascal's triangle), 108–9 (on binomial coefficients), 150 (on an algorithm for figurate numbers), 180ff. and 331ff. (on tables of combinations); BL Add. MS 6783, ff. 403ff. ('De numeris triangularibus et inde de progressionibus arithmeticis'); BL Add. MS 6789, f. 54v (on the properties of 4, 7 and 9).

[64] This is the case of Cardano: see I. Maclean, 'Cardano on the immortality of the soul', in G. Canziani and M. Baldi (eds), *Cardano e la tradizione dei saperi* (Milan, 2004), pp. 191–208.

[65] E.g. BL Add. MS 6782, 362ff.

[66] See Maclean, 'Cardano on the immortality of the soul', p. 206; Edward Rosen, 'Harriot's science: the intellectual background', in Shirley (ed.), *Thomas Harriot. Renaissance scientist*, p. 13.

mathematician, Harriot very rarely made a mistake, that as a reckless philosopher, he blundered more often, and that as a mortal man, he erred signally. The work was described by Torporley as a refutation of the 'atomistical pseudophilosophy', which Harriot is said to have revived and which, Torporley avers, merits stern reproach as well as anathema.[67] This suggests that Harriot was known to hold views which were religiously unacceptable to an early modern Christian. But there are indications which might lead us in the opposite direction. Bacon, whom I quoted earlier on the soundness of the atomistical doctrine so abhorred by Torporley, was aware of the implications of this doctrine for divine involvement in the sublunary world and rebuts the charge of atheism by an extraordinary analogy in which God is made to play the part of a devious Machiavellian Renaissance prince:

> Neither does [Democritean philosophy, i.e. atomism] call in question or derogate from diuine Prouidence, but highly confirme and exalt it. For as in ciuill actions he is the greater and deeper pollitique, that can make other men the Instruments of his will and endes, and yet neuer acquaint them with his purrpose: So as they shall do it, and yet not knowe what they doe, then hee [who] imparteth his meaning to those he employeth: So is the wisdome of God more admirable, when Nature intendeth one thing, and Prouidence draweth forth another; then if hee had communicated to particular Creatures and Motions the Characters and Impressions of his Prouidence.[68]

This justification for ignoring the question of final cause is not found in Harriot; he does however indicate what his attitude was to the intervention of the divinity in his creation in the letter to Mayerne, which I quoted at the beginning of this lecture, even if he does not expatiate on it.

Speculation about any thinker's intimate religious convictions is especially dangerous, so, rather than pursue this line of enquiry, I should like to end on two clues which relate to his research rather than his personal faith. The first of these is a sentence from his petition to the Privy Council of 16 December 1605 for his release from imprisonment in connection with the investigation of the Gunpowder Plot: 'all that know me can witness that I was alwayes of honest conversation and life … contented with a private life for the love of learning that I may study freely'.[69] The second clue comes from a letter to Kepler of 13 July 1608, in which the phrase 'to study freely' recurs: 'things are of in such a state here that I have not hitherto been allowed to philosophise freely; we remain here stuck in the

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[67] Shirley, *Thomas Harriot. A biography*, pp. 4–5, 472–4; Sion Coll MS arc L 40.2/E.10, ff. 7–12.

[68] Bacon, *The advancement of learning*, p. 87.

[69] Cited by G. Batho, 'Thomas Harriot and the Northumberland household', in Fox (ed.), *Thomas Harriot*, pp. 28–47 (p. 36).

mud. I hope that Almighty God will soon put an end to this state of affairs'.[70] Phrases such as 'Libere philosophari' and 'libertas philosophandi' have a long history, to which I have briefly alluded: in the second half of the sixteenth century, they became shibboleths of independent thought throughout Europe. In his 1563 edition of the works of Marsilio Ficino's pupil Francesco Cattani da Diacceto, a member of the Florentine Academy in the second half of the fifteenth century, Theodor Zwinger of Basle (1533–88) dates the practice, if not the use, of the term 'libertas philosophandi' to the humanist recovery of Platonic and ancient hermetic knowledge in that same Academy. The 'Christian philosopher' Nicolaus Taurellus (1547–1606), who taught natural philosophy and medicine at Altdorf, describes 'free philosophising' as the product of a university training in the exercise of judgement in his book on the peripatetic Andrea Cesalpino, which appeared in 1593; Francis Bacon does the same in his *The advancement of learning* a decade or so later. At about the same time, Galileo adapts the Athenian Platonist Alcinous's adage about philosophy and freedom of birth to justify the unfettered meditations of the philosopher. In his *Apologia pro Galileo* of 1622, Tommaso Campanella (1568–1639) claims that freedom of thought is specifically a feature of Christian culture and according to him, as well as the Lutheran philosopher Jakob Martini of Wittenberg (1570–1649), the book of nature was configured by God in such a way as to allow human enquirers, whether pagan or Christian, the freedom to read it. Thus, Aristotle was indeed able to acquire true knowledge about the world, and his successors could continue to make new discoveries about it by their own free observation of it (a point made also by both Kepler and Galileo).[71]

It seems to me that Harriot consistently conducted his enquiries in that spirit; this, however, does not entail that we have to distrust his explicit declarations about his adherence to orthodox Christian faith. From his writings about numbers, words and atoms, I see no evidence that he was interested in their mystical powers

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[70] J. Kepler, *Gesammelte Werke*, vol. 16: *Briefe 1607–11*, ed. M. Caspar (Munich, 1954), pp. 172–3 (no. 497): 'ita res se habent apud nos ut non liceat mihi adhuc libere philosophari. Haeremus adhuc in luto. Spero Deum Optimum Maximum his brevi daturum finem.'

[71] C. Gilly, 'Zwischen Erfahrung und Spekulation: Theodor Zwinger und die religiöse und kulturelle Krise seiner Zeit', *Baseler Zeitschrift für Geschichte und Altertumskunde*, 79 (1979), 132–3; N. Taurellus, *Alpes caesae, hoc est, Andreae Caesalpini Itali monstrosa et superba dogmata discussa et excussa* (Frankfurt, 1597), sig. *4v; Bacon, *The advancement of learning*, p. 28; J. Martini, *Exercitationum metaphysicarum libri duo* (Leipzig, 1608), sig. a5–6; R.B. Sutton, 'The phrase "libertas philosophandi"', *Journal of the history of ideas*, 14 (1953), 310–316: M.A. Stewart, '"Libertas philosophandi": from natural to speculative philosophy', *Australian journal of politics and history*, 40 (1994), 29–46 (both with references to Renaissance figures); I. Maclean, 'The "Sceptical crisis" reconsidered: Galen, rational medicine and the *libertas philosophandi*', *Early science and medicine*, 11 (2006), 247–74. I am grateful to Isabelle Pantin for pointing out that the epigraph of Rheticus's *Narratio prima* of 1540 bears the Alcinous adage, and that Kepler adopted this as a device from the publication of the *Mysterium cosmographicum* in 1596 onwards.

or the meanings of the patterns in which they occur, and much that points in the direction of a man capable of highly abstract mathematical thought which was neither linked to the demands made upon him by patrons nor to an ideological commitment of any sort. In respect of the three historiographical debates to which I alluded at the beginning of this chapter, it would seem to me that Harriot does not adhere to an 'emblematic world view': he treats nature as an object of enquiry which does not necessarily yield up mystical knowledge and looks on the patterns of mathematics as an area for free investigation, not as a means of contemplating the godhead. In this, his writing does not support the Cunningham-French view that the attributes of God are part of natural-philosophical enquiry at this time (which is not to say that Harriot's mental horizons are wholly and uniquely secular). The manner in which mathematical problems are set out in his surviving manuscripts would seem to me to further support the view that they were not driven by practical concerns but by the speculations of a gifted mathematician. I therefore come to the conclusion that Harriot's work on combinations is scarcely marked at all by the social, political and religious context from which it arose (which is not to say that his work on alchemy or on practical mathematics is unmarked in the same way); that he is not a disciple of Bruno; that he did not feel obliged to consider the attributes of the deity while engaging in natural philosophy; that he was able to speculate mathematically without linking such speculation to practical ends; and that he, like many of his contemporaries (including the Bacon of *The advancement of learning*), was capable of compartmentalizing his mind and of according different modes and degrees of commitment to different areas of his mental universe. As such, it seems appropriate to end by quoting one of Harriot's more enigmatic remarks, which he wrote on a scrap of paper and pasted on to a page of propositions about infinites: its last line could apply as much to this chapter as to others in which scholars have incautiously set themselves the task of interpreting his elusive texts:

> much ado about nothing
> great warres and no blows
> who is the foole now?[72]

[72] BL Add. MSS 6785, f. 436v, cited by Gatti, 'The natural philosophy of Thomas Harriot', p. 79; Mandelbrote, 'The religion of Thomas Harriot', p. 258 reads 'wavves' for 'warres'.

Chapter 5
Thomas Harriot as an English Galileo: The Force of Shared Knowledge in Early Modern Mechanics[1]

Matthias Schemmel

Harriot, Galileo and preclassical mechanics

It has from time to time been pointed out that Thomas Harriot's work displays striking similarities to Galileo Galilei's. Harriot constructed telescopes independently of and even prior to Galileo, for example, and used them to observe the moon, sun spots and later – after having read Galileo's *Sidereus nuncius* – also Jupiter's satellites. His scientific agenda was similar to Galileo's and included work in mechanics, optics, hydrodynamics and magnetism. Historians of mathematics also consider Harriot as one of the early modern pioneers of algebra.[2]

In view of this, one might expect that Harriot would be widely recognized as an important figure in the history of modern science. This, however, is not the case. While various aspects of Harriot's work have been the subject of specialized studies, more general accounts on the history of science and the early modern scientific revolution rarely mention his name. The reason seems obvious. Apart from a small book on 'the new found land of Virginia',[3] based on his experience as an explorer of the New World in the service of Sir Walter Ralegh, Harriot did not publish any of his scientific findings. He also had no disciples or students who outlived him to further develop his science. His work thus seems to be a

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[1] This chapter is a slightly revised version of the following article: 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', *Physics in perspective*, 8 (2006), 360–80. I thank the British Library in London for permission to reproduce Figures 5.1, 5.5, 5.6 and 5.8, and the Biblioteca Nazionale Centrale in Florence for permission to reproduce Figures 5.3 and 5.10.

[2] For a full account of Harriot's life and work, see J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983). See also the papers collected in J.W. Shirley (ed.), *A source book for the study of Thomas Harriot* (New York, 1981) and R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000). For an assessment of Harriot's work in algebra, see J. Stedall, 'Rob'd of glories: the posthumous misfortunes of Thomas Harriot and his algebra', *Archive for history of exact sciences*, 54 (2000), 455–97.

[3] T. Harriot, *A briefe and true report of the new found land of Virginia* (London, 1588).

dead end in the history of science; his influence on the future development of science appears to be negligible. As such, the study of his work may seem to be of provincial interest only, offering little more than the addition of curious details to our picture of Elizabethan society. How, after all, can the thoughts and insights of a historical figure contribute to the history of science if they never entered the mainstream of developments that eventually formed modern science?

Such considerations are rooted in a widespread image of the history of science as a sequence of contributions by individual scientists, with innovations in science then occurring through their insights and discoveries when presented to the scientific community. There can be no doubt, of course, that thinking takes place in the minds of individuals and new insights are achieved by individuals. But the thinking of an individual is governed to a large degree by knowledge that is shared with his or her contemporaries, or certain specialized groups of contemporaries. This shared knowledge thus gives meaning to an individual's thought constructs and their variations.

The shared framework of Harriot's and Galileo's work on mechanics was not yet that of classical mechanics and may be termed 'preclassical mechanics'.[4] This term should not be taken to suggest a coherent framework of mechanics, such as that provided later by classical mechanics. Preclassical mechanics emerged from the application of traditional theoretical tools and concepts to new and challenging objects of study provided by early modern engineering and warfare. Practical mathematicians and engineer-scientists such as Guidobaldo del Monte (1545–1607), Simon Stevin (1548–1620), Thomas Digges (1546?–95), Galileo and Harriot – to name but a few of them – approached contemporary technological problems on the basis of a heterogeneous corpus of shared knowledge, drawing on such sources as Aristotelian physics, other ancient traditions, medieval calculation techniques and, most importantly, the practical knowledge of engineers and gunners. The integration of these heterogeneous components of knowledge necessitated its restructuring, eventually leading to a radical change in basic mechanical concepts such as space, time and force, and resulting in the establishment of classical mechanics.

To reconstruct the shared knowledge of early modern mechanics, we therefore must study not only the work of the heroes in the traditional accounts of the scientific revolution of the seventeenth century but also the work of lesser-known individuals such as Harriot.[5] We shall see that studying Harriot's work in

[4] I adopt the term 'preclassical mechanics' from P. Damerow, G. Freudenthal, P. McLaughlin and J. Renn, *Exploring the limits of preclassical mechanics*, 2nd edn (New York, 2004).

[5] For a comprehensive reconstruction, analysis and interpretation of Harriot's work on motion, see M. Schemmel, *The English Galileo. Thomas Harriot's work on motion as an example of preclassical mechanics* (Dordrecht, 2008). An independent, concise account is given in J.-J. Brioist and P. Brioist, 'Harriot, lecteur d'Alvarus Thomas et de Tartaglia',

mechanics and comparing it with Galileo's will allow us, by way of example, to address questions of the following kind:

- what aspects of a scientist's work reflect structures of the shared knowledge and what aspects represent the peculiarities of an individual scientist's work?;
- what alternative pathways were open to contemporary scientists in their approaches to shared problems?;
- to what extent do the peculiarities of an individual scientist's work influence its outcome?

Further, ensuing from these, we can ask questions concerning the long-term development of science such as:

- do the peculiarities of an individual scientist's work lead to diverging developments in science? Would we have a completely different physics today had there been no Galileo?; or
- do the lines of alternative development in science converge in such a way that the long-term development of science is not affected by local deviations owing to the peculiarities of an individual scientist's work?

Harriot's work in mechanics seems particularly suitable as the basis for a comparison with Galileo's for at least three reasons. First, Harriot's and Galileo's work on motion was carried out independently. Hence, while Harriot learned of Galileo's work on astronomy through the publication of *Sidereus nuncius* in 1610, he could not have known about Galileo's work on mechanics, because it was published only after Harriot's death in 1621. There is also no evidence that the two had any personal contact or corresponded with each other. Further, as I will show later, the inferential pathways of their arguments were completely different. Second, Harriot's work on mechanics contains insights that became cornerstones of classical mechanics. I call these 'points of contact' between preclassical and classical mechanics. Harriot's work thus covers a developmental range equivalent to Galileo's. Third, while Harriot did not publish anything on mechanics, the manuscript evidence that documents his work is richer than that of most of his contemporaries. Thus, among the more than 8,000 folio pages of Harriot's manuscripts that are preserved partly in the British Library in London and partly at Petworth House in Sussex, I have identified about 200 folio pages that deal with the problems of projectile motion and of free fall.[6]

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in J. Biard and S. Rommevaux (eds), *Mathématiques et théorie du mouvement. XIVe–XVIe siècles* (Villeneuve d'Ascq, 2008), pp. 147–72.

[6] A total of 180 of these folio pages constitute the basis for the analysis given in Schemmel, *The English Galileo*. For a discussion of the selection, see in particular p. 391.

I shall compare four different aspects of Harriot's work on projectile motion with Galileo's, arguing in the following four sections that:

- it supports the idea that common challenging objects of study emerged from contemporary engineering experience and played a decisive role in shaping the course of early modern mechanics;
- it shows that several of the insights that became cornerstones in the development of classical mechanics, the 'points of contact' with classical mechanics, were attained not only once, and not only by Galileo;
- it suggests that there was a shared theoretical framework that largely defined the space of possible solutions to the shared problems; and
- it suggests that, despite the many degrees of freedom within the argumentative structure, the available knowledge fostered certain definitive results.

In my concluding section, I shall return to the questions I raised above concerning the long-term development of science.

Common challenging objects of study

If Harriot's and Galileo's contributions to mechanics were isolated intellectual constructs, we should expect their work to differ even with respect to the objects of study and the questions they raised about them. There are striking parallels, however, with respect to both of these aspects of their work. The motion of a projectile, the motion of a ball on an inclined plane, the flow of water and the force of percussion, for example, were central topics of study for both Harriot and Galileo. What is more, the specific questions they raised about them were virtually identical.

Consider the case of projectile motion. A central concern here for both Harriot and Galileo was the geometrical shape of a projectile's trajectory and, knowing its shape, to determine the dependence of a projectile's range on its angle of projection. This striking similarity becomes understandable if we recognize that a projectile's trajectory was one of several common challenging objects of study in early modern mechanics.[7] These were mechanical situations or phenomena occurring in the context of early modern engineering and the practice of warfare before they became subjects of theoretical study. This practical tradition did not serve simply as a source of inspiration or motivation to study one or another object; rather, it provided essential knowledge about the properties and behaviours of these objects of study that consisted of the accumulated experience of the practitioners. This practical knowledge was precisely what transformed them into challenging objects

[7] On the notion of 'challenging object', see, for example, J. Büttner, 'The pendulum as a challenging object in early modern mechanics', in W.R. Laird and S. Roux (eds), *Mechanics and natural philosophy. Accommodation and conflict* (Dordrecht, 2008), pp. 223–37.

of study, since it had to be captured by their theoretical treatment. The challenging objects of study accordingly challenged not only the abilities of early modern scientists but also the traditional concepts and structures of theoretical knowledge.

In fact, while projectile motion was mentioned and at times discussed in medieval science, a projectile's trajectory became a challenging object of study only in early modern times when artillery became increasingly important in warfare. The possibility of independently varying a projectile's angle of projection (that is, the elevation of the gun) and its initial velocity (that is, the powder charge) led to refined empirical knowledge of the shape of its trajectory, which served as a set of constraints on any theory of projectile motion.

Practical knowledge about artillery consisted initially of the professional knowledge of a specialized group of practitioners, the gunners. However, owing to the spate of practical manuals on gunnery that appeared during the course of the sixteenth century,[8] this practical knowledge also became available to others, including theoreticians like Harriot and Galileo who had no personal experience with artillery.

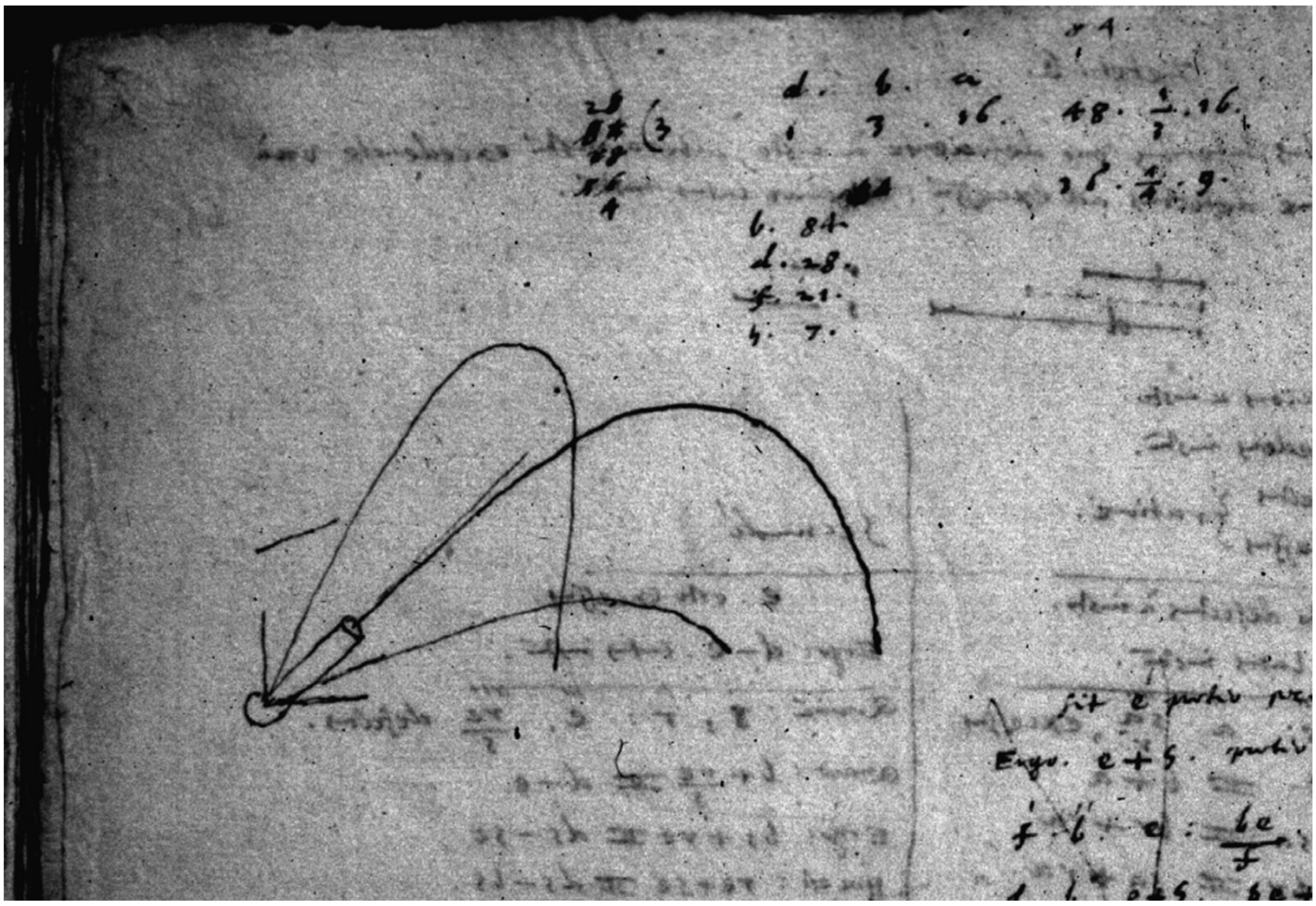

Figure 5.1 Harriot's sketch of three projectile trajectories. British Library Add. MS 6782, f. 465 verso. By permission of the British Library

[8] See, for example, W. Bourne, *The arte of shooting in great ordnaunce* (London, 1587).

Figure 5.1 shows three projectile trajectories that Harriot sketched in his manuscripts, which clearly reflect crucial aspects of the practitioner's knowledge about their shape. Consider, in particular, the dependence of the shape on the angle of projection. We see that there exists an angle somewhere around 45° for which the projectile's range is a maximum. We also see that for larger angles, the projectile goes higher. Thus, the practitioner's knowledge as reflected in this graphical representation clearly put severe restrictions on the set of possible solutions to a theoretical description of projectile motion.

Thus, the challenging objects of study and the knowledge they embodied may account for many similarities between Harriot's and Galileo's work. However, there are also differences between their work concerning both the importance they attached to a particular object of study and their assessment of the knowledge that a particular object of study embodied. For Harriot, as we shall see, it follows that the asymmetry of a projectile's trajectory was an essential feature of it that had to be captured by theory, while for Galileo it merely represented an accidental feature of it. Furthermore, we shall encounter later an object of study to which Galileo attached great importance; Harriot, on the other hand, never referred to it.

Points of contact with classical mechanics

Harriot's and Galileo's studies of projectile motion indicate, by way of example, that work on common challenging objects of study led to similar insights that could function as points of contact between preclassical and classical mechanics. One such point of contact was the insight that the shape of a projectile's trajectory is parabolic.

According to classical mechanics, a projectile's trajectory in a vacuum results from the composition of two motions: an inertial motion along its line of projection and an accelerated motion of free fall vertically downward. Since

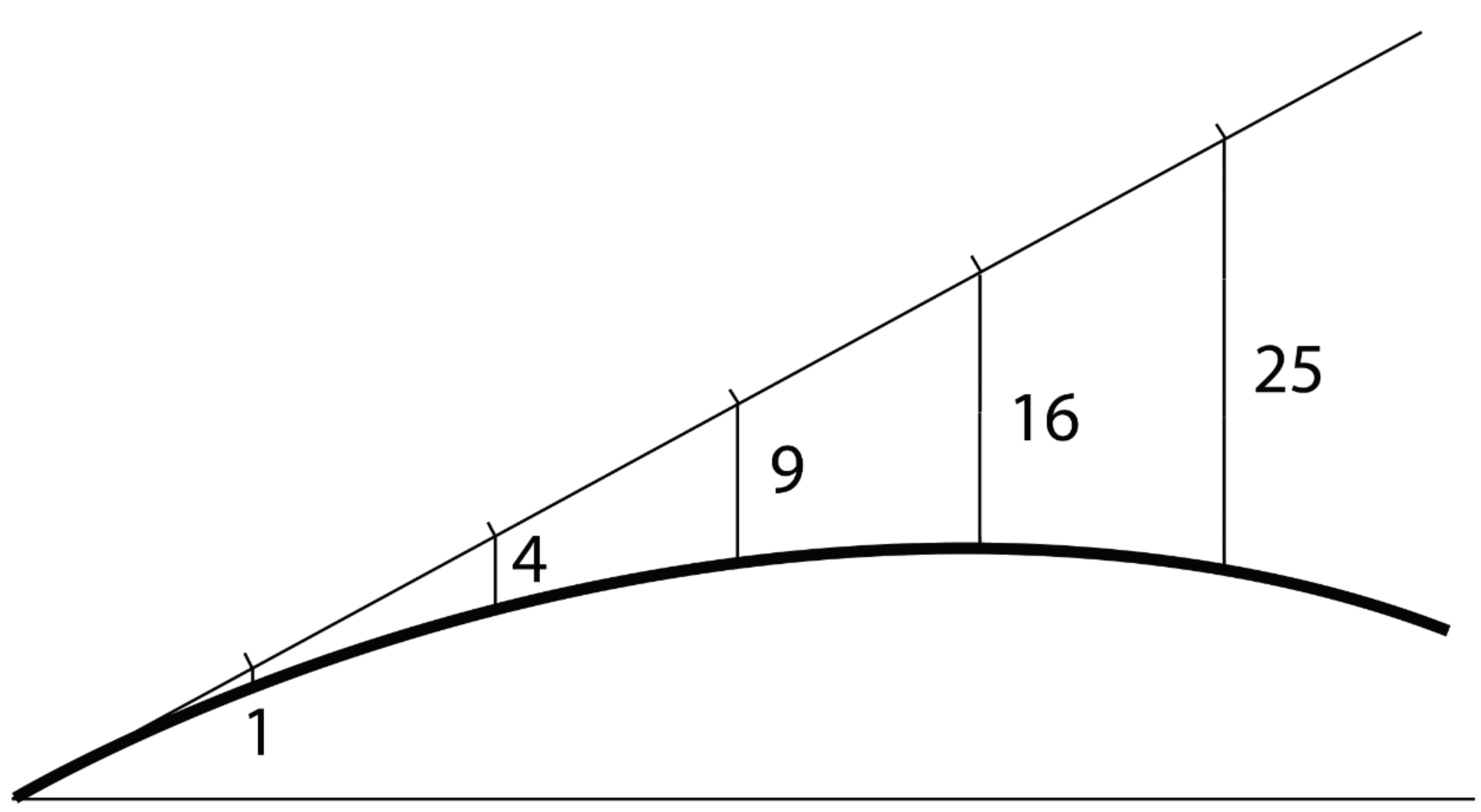

Figure 5.2 Author's sketch of the construction of a projectile trajectory (bold line) according to classical mechanics

the laws governing these two fundamental kinds of motion are known, points on a projectile's trajectory can be constructed geometrically by considering the distances traversed by a projectile in undergoing both motions in equal intervals of time, as shown in Figure 5.2. The uniform inertial motion along its line of projection is represented by equidistant points on the oblique line, marking the distances the projectile traverses in equal intervals of time, while the distances it traverses in free fall following the beginning of its motion are measured vertically downward from these points and increase quadratically with time, as indicated. The points constructed in this way thus give the positions of the projectile at equal intervals of time, and its trajectory, represented by the bold line in the figure, is given by joining these points in a smooth curve.

Now, while Galileo attained the insight that a projectile's trajectory is parabolic, he was unable to derive its trajectory along the lines sketched above. That is precisely why I have called this insight a 'point of contact' with classical mechanics and not a first result of classical mechanics. In fact, the preclassical character of Galileo's insight becomes particularly evident when considering one of his unpublished working notes on projectile motion, as reproduced in Figure 5.3, which shows a ball running down a nearly vertical inclined plane and then being deflected obliquely, its subsequent trajectory being given by the curved, dashed line.[9] We can reconstruct this trajectory as shown in Figure 5.4, where the origin of the projectile is now taken to be in the lower left-hand corner. Its motion results from the composition of two motions, one along the line of projection and the other vertically downward. This construction looks similar to the one of classical mechanics (Figure 5.2), but there is one major difference. In Galileo's preclassical construction, the projectile's motion along its line of projection is not uniform inertial motion but decelerated motion. In other words, the spaces traversed along the oblique line in equal intervals of time decrease successively. The projectile's motion along its line of projection appears to be modelled by analogy with the motion of a ball on an inclined plane.

This analogy was not just a fleeting idea for Galileo. Even in his final work in mechanics, his *Discorsi* of 1638,[10] he still expressed the view that the motion of a projectile along its line of projection is decelerated motion whenever this line is directed upwards. This of course is fallacious according to classical mechanics. Galileo's preclassical conception coincides with that of classical mechanics only in the case of horizontal projection, that is, when the inclination of the plane is zero degrees and no deceleration owing to gravity occurs. For oblique projection, however, Galileo's preclassical conception results in an asymmetric curve, as

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[9] Naylor was the first to interpret this drawing as a theoretical analysis of projectile motion: see R.H. Naylor, 'Galileo's theory of projectile motion', *Isis*, 71 (1980), 550–70. I follow the interpretation given by Damerow et al., *Exploring the limits of preclassical mechanics*, pp. 216–20.

[10] G. Galilei, *Discorsi e dimostrazioni matematiche. Intorno à due nuoue scienze attenenti alla mecanica i movimenti locali* (Leyden, 1638).

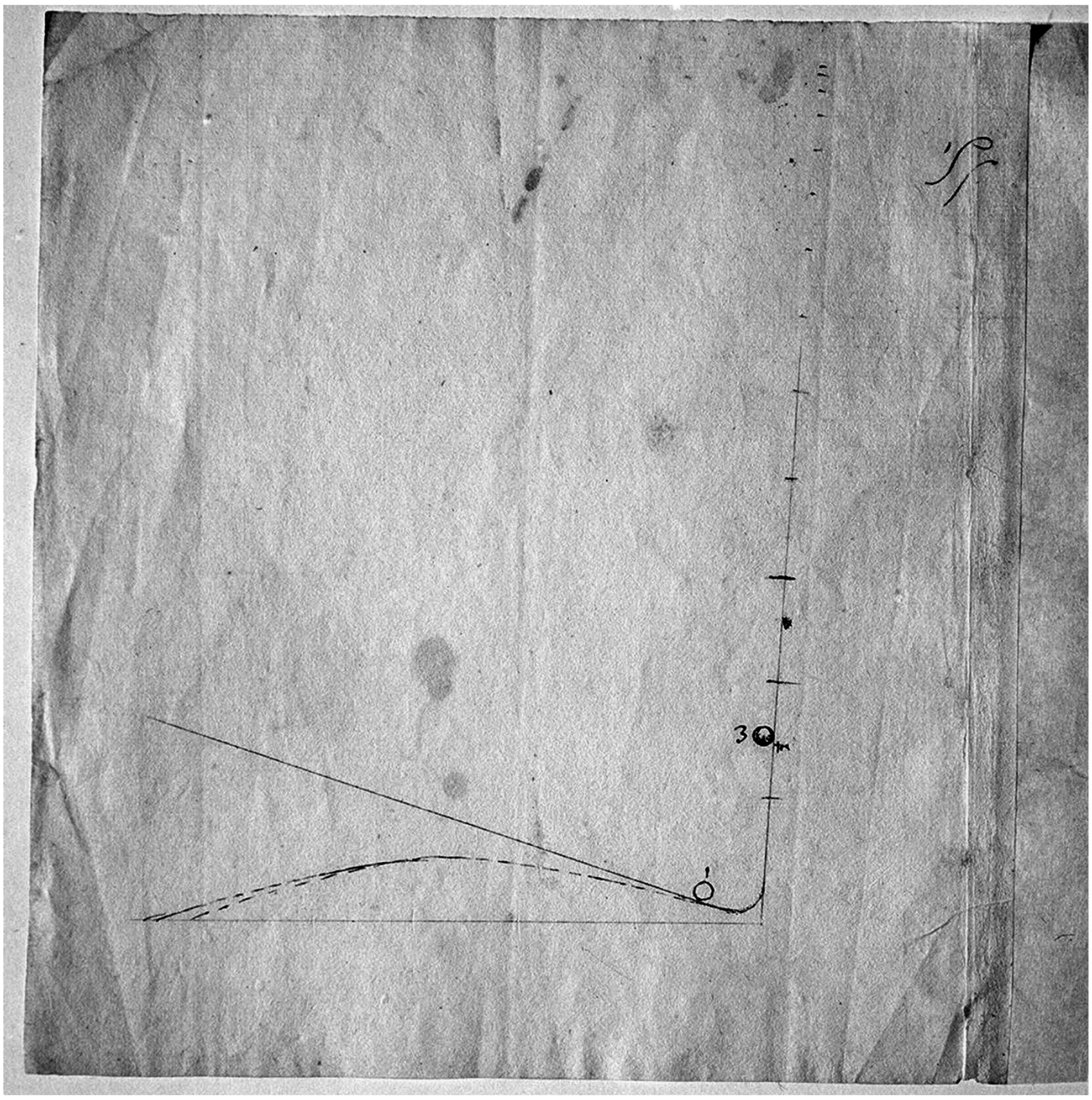

Figure 5.3 Galileo's drawing of the oblique projection of a ball. Biblioteca Nazionale Centrale di Firenze, Galileo MS 72, f. 175 verso. By concession of the Ministero per i Beni e le Attività Culturali della Republica Italiana/Biblioteca Nazionale Centrale di Firenze

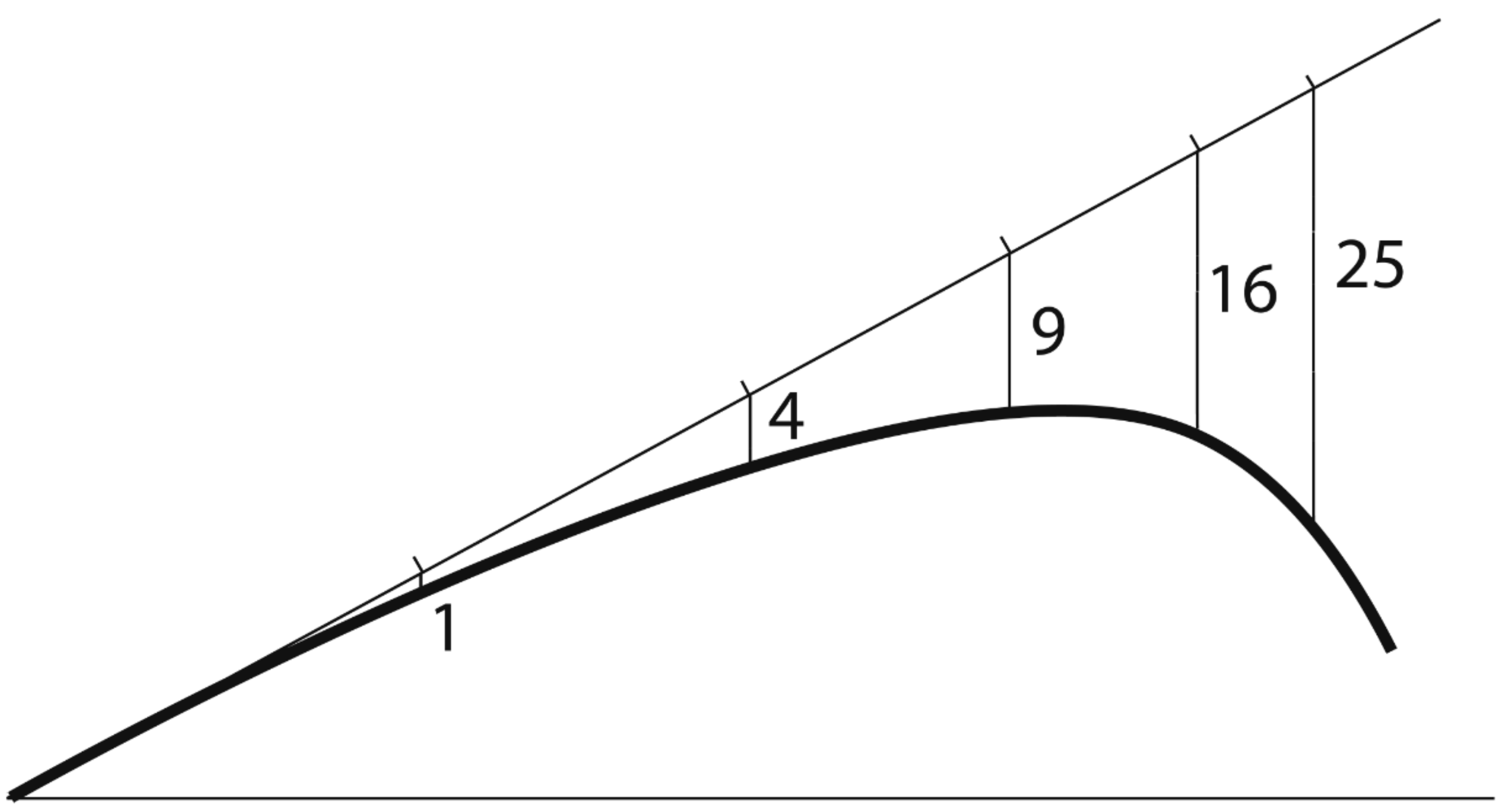

Figure 5.4 Author's reconstruction of the projectile trajectory shown in Galileo's MS 72, f. 175 verso (see Figure 5.3)

shown in Figure 5.4, which is at odds with the classical result. As such, Galileo was confronted with a contradiction between his inclined-plane analogy for projectile motion and his claim that a projectile's trajectory is an upright parabola for all angles of projection. He was never able to resolve this contradiction and consequently his proof of the parabolic shape of a projectile's trajectory remained incomplete in his *Discorsi*.[11]

Let us compare these findings with Harriot's analysis of projectile motion. In Figure 5.5 an excerpt of one of the more than 100 folio pages of Harriot's manuscript notes on projectile motion is reproduced. Below a drawing showing the construction of a curve, Harriot wrote:

> The species of the line that is made upon the shot of poynt blanke is as is here described & is a parabola as of the upper randons.[12]

Harriot's 'shot of poynt blanke [point blank]' denotes a horizontal shot, while 'upper randons' refers to shots at an elevation above the horizontal. He evidently constructs the projectile's trajectory as I have described earlier, that is, by compounding two motions traversed in equal intervals of time, a uniform horizontal motion and an accelerated motion of free fall vertically downward that increases quadratically with time, as indicated by his numbers 1, 4, 9 and 16.

Harriot must have written these notes before 1621,[13] the year of his death. They document his state of knowledge of the law of free fall and of the parabolic shape of a projectile's trajectory to the same extent that Galileo's *Discorsi* of 1638 documents his state of knowledge at that time. In fact, Harriot's construction reflects both the same insights and the same weaknesses as Galileo's exposition, since Harriot derives the parabolic shape of the trajectory only for the case of horizontal projection.

How, then, did he envision the case of oblique projection? Fortunately, we can answer this question, because the above folio is part of a group of folios in which Harriot deals with projectile motion for arbitrary angles of projection. In particular,

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[11] To prove the parabolic shape of projectile trajectories for projections at oblique angles, Galileo considered horizontal projections which he could prove yielded parabolic trajectories. He then argued that horizontally projected objects hit the ground at oblique angles and claimed the motion to be reversible: see Galileo Galilei, *Le opere*. *Nuovo ristampa della edizione nazionale 1890–1909*, vol. 8 (Florence, 1968), p. 296. His claim, however, remained unproven. The deficiency in Galileo's argument was pointed out by Descartes in his famous critique of Galileo's *Discorsi*; see R. Descartes, *Oeuvres complètes*. *Nouvelle présentation*, ed. C. Adam and P. Tannery, vol. 2 (Paris, 1988), p. 387, letter 146. It was also discussed later by E. Wohlwill, 'Die Entdeckung Des Beharrungsgesetzes II', *Zeitschrift für Völkerpsychologie und Sprachwissenschaft*, 15 (1884), 70–135, esp. 111ff.

[12] BL Add. MS 6789, f. 67r.

[13] Shirley dates these notes to 1607 on the basis of Harriot's handwriting. See Shirley, *Thomas Harriot*, p. 261.

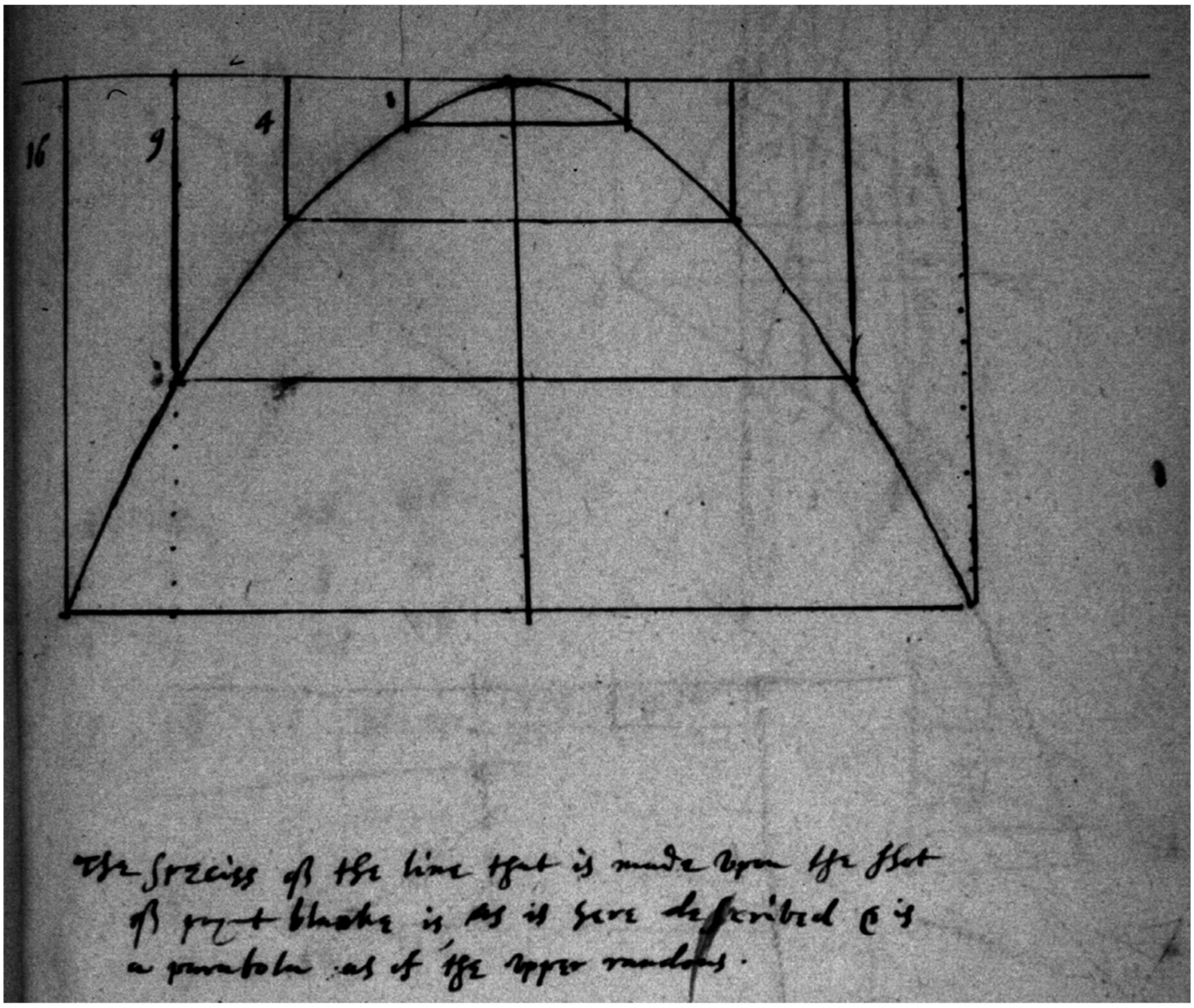

Figure 5.5 Harriot's drawing of an upright parabola representing the horizontal projection of a projectile. British Library Add. MS 6789, f. 67 recto. By permission of the British Library

we find on one folio page trajectories for shots at angles of 15°, 30°, 45°, 60°, 75° and 90°. It was not clear at first how Harriot obtained these trajectories, since no construction lines were visible, but by illuminating the folio with raking light (as was done for the photograph shown in Figure 5.6), an abundance of construction lines carved into the paper but not drawn in ink were revealed. The trajectories and those construction lines that are crucial for understanding how Harriot determined the trajectories are reproduced in Figure 5.7. According to my analysis, Harriot constructed the trajectories by taking the projectile's vertical motion to obey the law of free fall, and its motion along its line of projection to be decelerated motion analogous to upward motion along an inclined plane.[14] In other words, he adhered

[14] The quarter circles intersecting the oblique lines at equal distances from the origin mark the distances traversed in equal intervals of time by a projectile moving with uniform motion along its line of projection. Each vertical line beginning at an intersection point of an oblique line and a quarter circle, the oblique line itself, and the line segment perpendicular

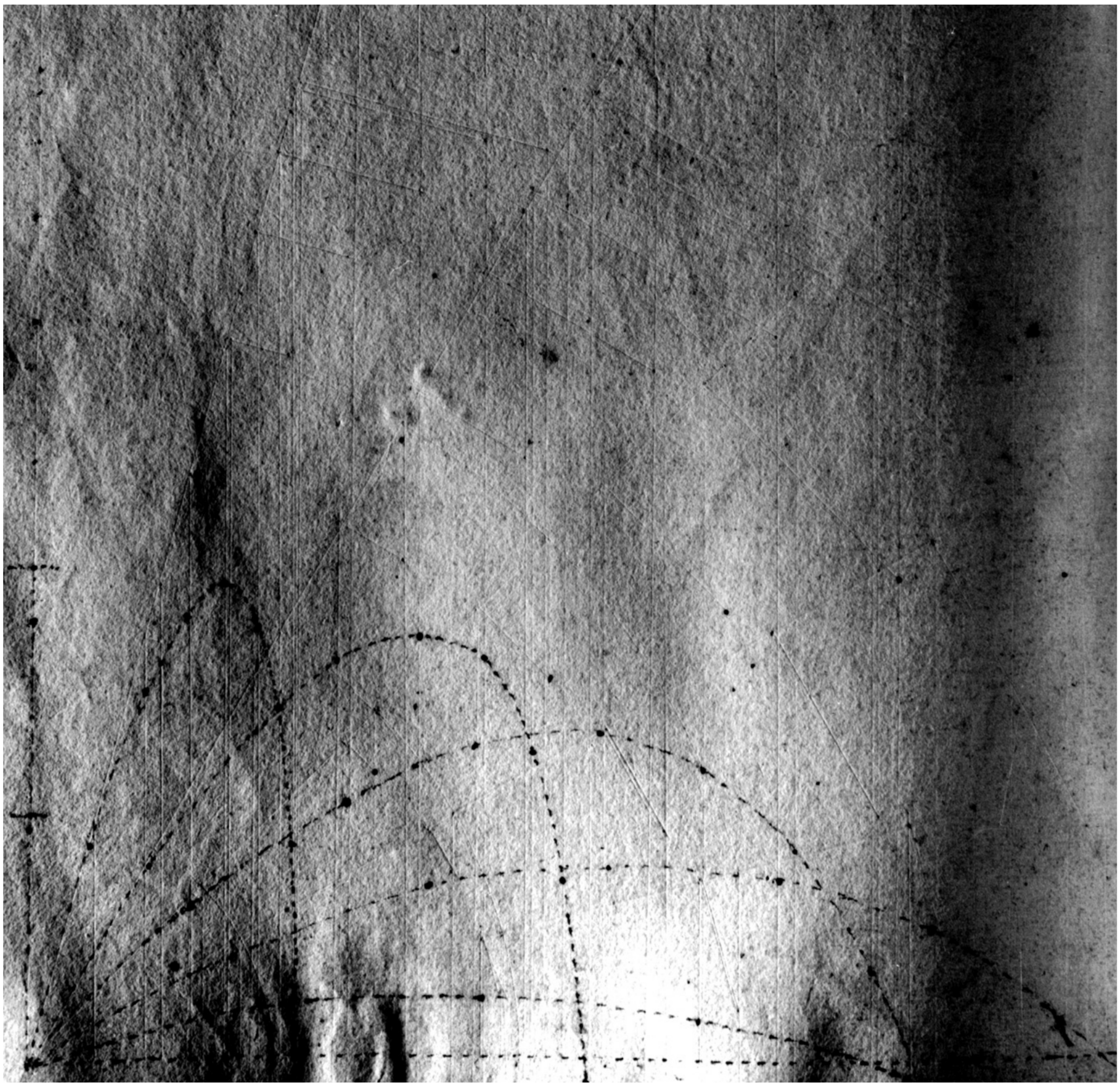

Figure 5.6 Raking-light photograph of projectile trajectories drawn by Harriot which reveals the faint construction lines he carved into the paper but did not draw in ink. British Library Add. MS 6788, f. 216 verso. By permission of the British Library

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to the oblique line form a right triangle. The lengths of the hypotenuses of successive right triangles along an oblique line increase quadratically. The leg of the triangle on the oblique line therefore also increases quadratically, its length being proportional to the sine of the angle of projection. This length is to be subtracted from the distance traversed in uniform motion to obtain the decelerated motion along the line of projection. From the points on an oblique line thus obtained, line segments are drawn vertically downward, which are equal in length to the hypotenuses of the right triangles. The lower endpoints of these vertical lines are points on the projectile's trajectory. Its motion along its line of shot is thus a quadratically decelerated motion, the deceleration varying as the sine of its angle of projection, and its vertically downward motion, which is equal to that along a plane inclined at 90°, is accelerated according to the law of free fall.

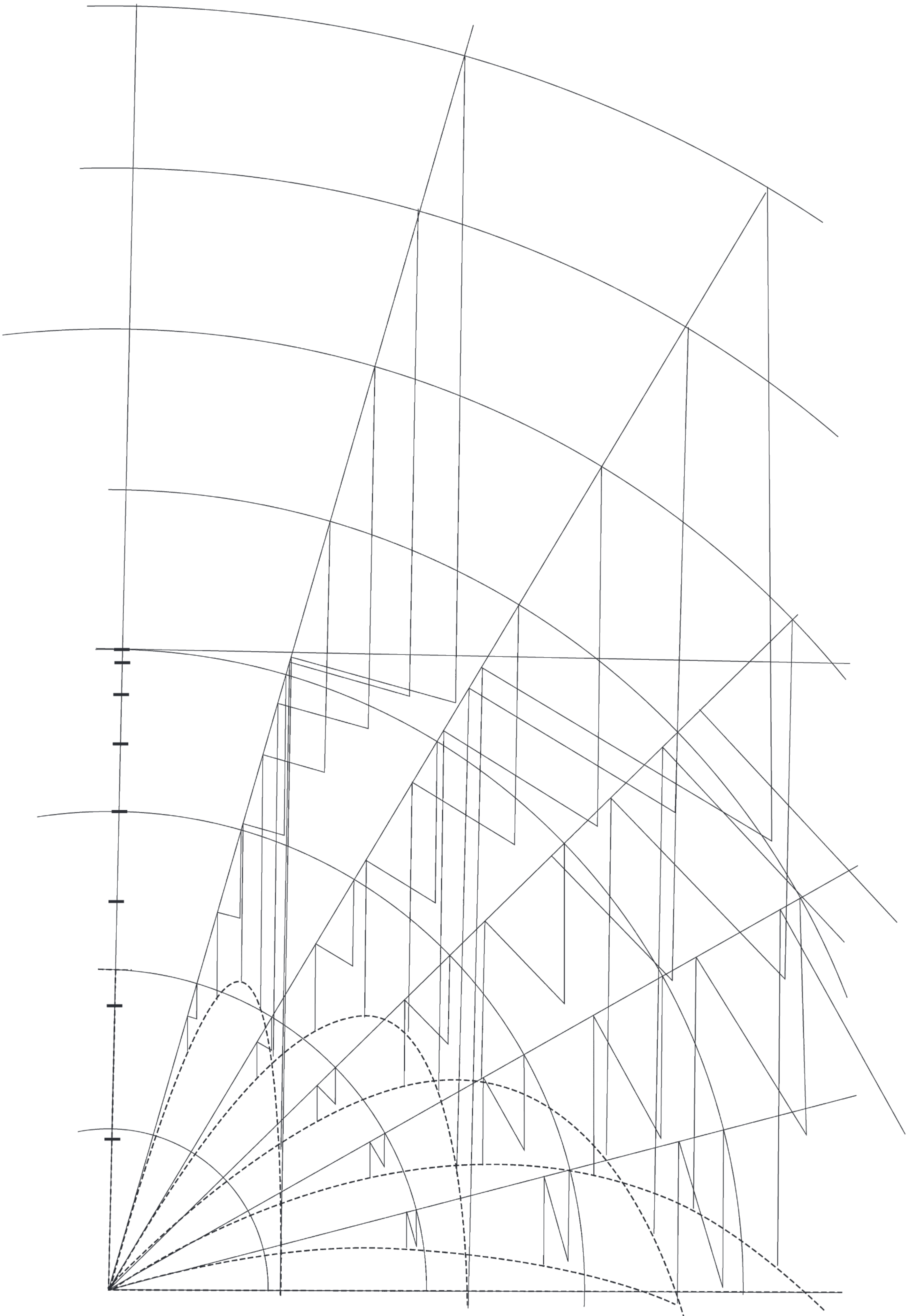

Figure 5.7 Author's reproduction of the six trajectories (dotted lines) Harriot drew and his construction lines (faint lines) for them as revealed in Harriot's MS. British Library Add. MS 6788, f. 216 verso (see Figure 5.6)

to the same inclined-plane conception of projectile motion that we discerned above in the work of Galileo.

The analogy to the motion along an inclined plane is even more obvious in Harriot's case than in Galileo's, since for Harriot the deceleration along the line of projection follows exactly the law of the inclined plane; that is, it varies as the sine of the angle of projection.

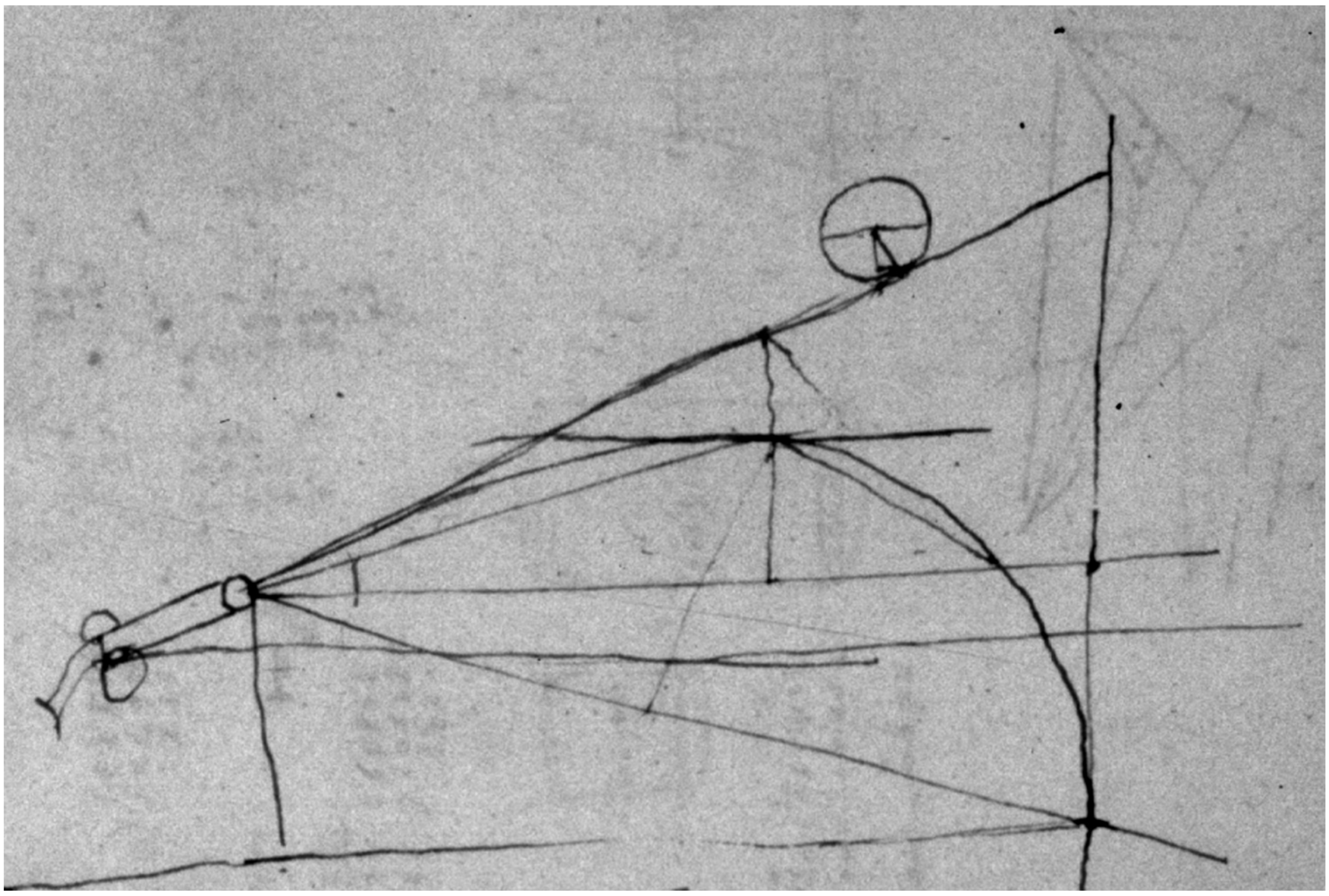

Figure 5.8 Harriot's sketch of a projectile trajectory revealing its analogy to motion on an inclined plane. British Library Add. MS 6789, f. 60 recto. By permission of the British Library

That for Harriot the motion of a projectile along its line of projection is equivalent to the motion of a cannon ball on an inclined plane is also clearly illustrated in the sketch reproduced in Figure 5.8. Harriot has depicted the cannon ball on its line of projection exactly as if it were moving on an inclined plane. Moreover, he has drawn a diagram inside the cannon ball that is reminiscent of constructions known from Pappus of Alexandria (*fl.* 300–350 AD), which were used to determine the effective weight of a ball on an inclined plane depending on its angle of inclination.

Shared knowledge as defining the space of possible solutions

For the case of projectile motion, therefore, we see that the same points of contact with classical mechanics emerged in the work of both Harriot and Galileo. In view of the manifold possibilities for dealing with projectile motion, this must be surprising. I now shall show how the similarities and differences between Harriot's and Galileo's solutions to the problem of the geometrical shape of a projectile's trajectory originated in a shared theoretical conceptualization of projectile motion.

There were a limited number of mathematical curves known in early modern times. Besides the straight line, these were mainly the conic sections; the parabola, hyperbola and ellipse (with the circle as a special case) were all proposed by various early modern mathematicians to describe a projectile's trajectory or parts of it.[15] But Harriot's and Galileo's task was not simply to describe the trajectory geometrically; it was to derive the geometrical shape from general principles of motion. Let us therefore take a closer look at the prevailing conception of motion in early modern times.

Its most important feature was the fundamental Aristotelian distinction between two kinds of motion: natural and violent. According to this distinction, the motion of free fall is natural motion, namely the motion of a heavy body striving to go toward its natural place, the centre of the universe. Violent or forced motion, by contrast, is the motion of a body being either pushed or pulled; the motion ceases when the force is no longer exerted. The detailed understanding of these two kinds of motion had undergone changes since medieval times. However, the fundamental distinction between the two persisted, and projectile motion was conceived to be somehow composed of them.

A suggestive way for describing a projectile's trajectory geometrically in terms of violent and natural motion was based on Euclidean straight lines and circles. The most prominent proponent of this view was the Italian mathematician Niccolò Tartaglia (1499/1500–1557), who proposed that a projectile's trajectory consisted of three parts: a straight line in the direction of the shot, representing violent motion; an intermediate segment of a circle; and a vertical straight line, representing the natural motion of free fall. These three parts of a projectile's trajectory could be referred to as a projectile undergoing purely violent, mixed and purely natural motion, as illustrated by the Spanish artillerist Luys Collado (fl. 1586) in his book *Platica manual de artilleria* of 1592 (see Figure 5.9).[16]

[15] Thomas Digges, for example, proposed all three kinds of conic sections to describe a part of a projectile's trajectory, depending on its angle of projection. See L. Digges and T. Digges, *An arithmeticall militare treatise, named Stratioticos* (London, 1579), pp. 186–8.

[16] Tartaglia himself, by contrast, conceived of the circular part as being traversed in purely violent motion, as he clearly expresses in his *Nova scientia*, first book, proposition V. See S. Drake and I.E. Drabkin (trans. and eds), *Mechanics in sixteenth-century Italy. Selections from Tartaglia, Benedetti, Guido Ubaldo, and Galileo* (Madison, WI, 1969), p. 80.

A construction such as this might provide a suggestive analogy between a projectile's dynamics and the geometrical shape of its trajectory, but it does not allow the latter to be derived from the former. However, an associate of Harriot, the English mathematician Thomas Digges (1546?–95), did formulate a method for actually deriving the geometrical shape of the curved part of a projectile's trajectory undergoing mixed motion. Digges introduced the Archimedean spiral, another mathematical curve known since Antiquity, as a model for generating curved motion and proposed to adapt it to the case of projectile motion:

> As *Archimedes* line Helicall or Spirall, is made by the direct motion of a pointe carried in a right line, while that right line is Circularly turned as Semidiameter vpon his Circles Center. So is this Artillery Helicall line of the Bullets Circuite created onely by two right lined motions becomming more or lesse *Curue*, according to the difference of their Angles occasioned by the seuerall Angles of Randon. Wherevpon by demonstration Geometricall a *Theorike* may bee framed that shall deliuer a true and perfect description of those *Helicall* lines at all Angles made betweene the Horizon and the Peeces line Diagonall.[17]

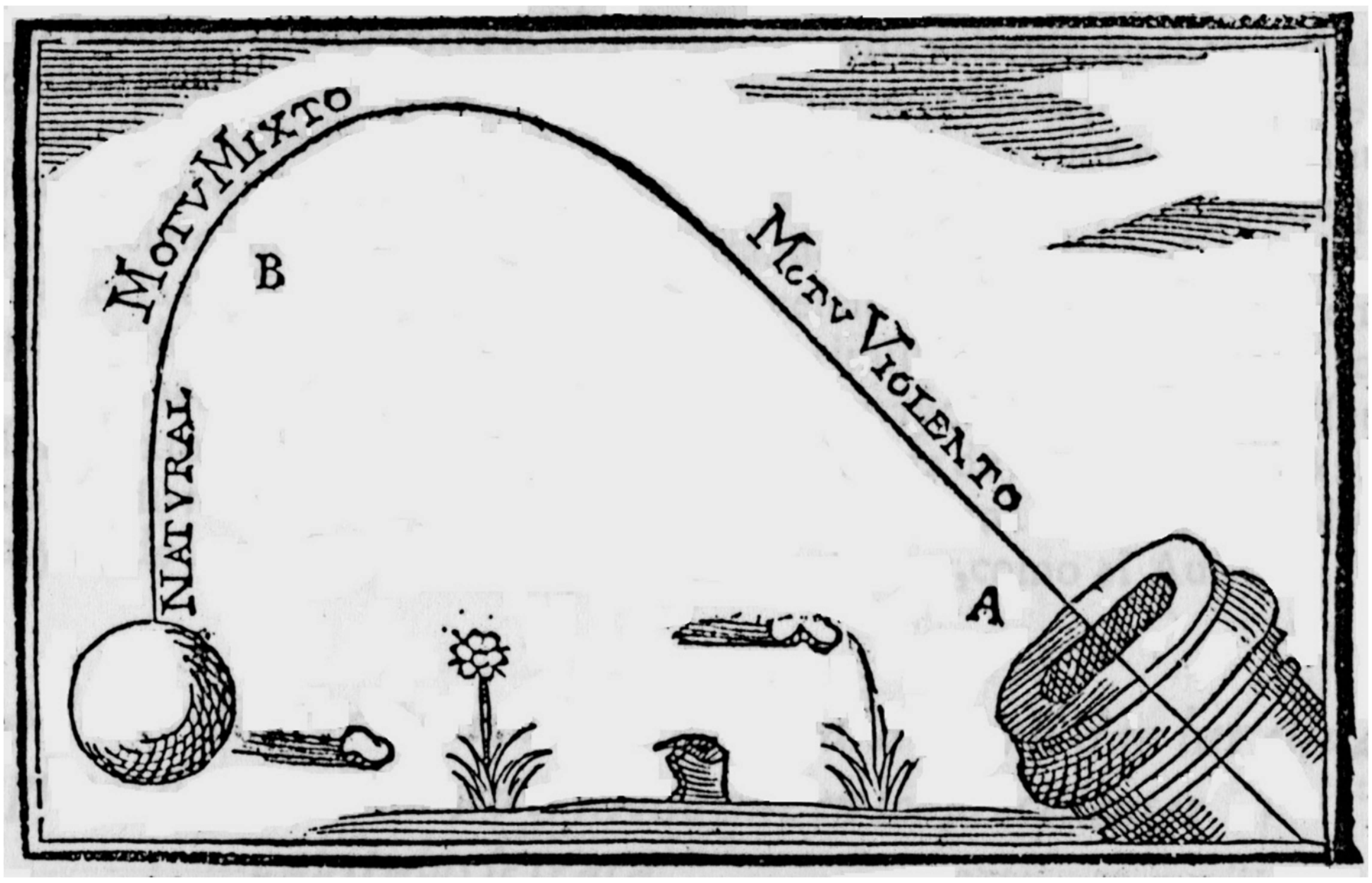

Figure 5.9 Luys Collado's representation of violent, mixed and natural motion as shown in a woodcut in his book *Platica manual de artilleria* (Milan, 1592), p. 40

[17] L. Digges and T. Digges, *A geometrical practical treatize named pantometria* (London, 1591), p. 168; the page number is misprinted and should be p. 184.

Galileo's and Harriot's pointwise constructions of a projectile's trajectory, as I described above, may be regarded as the realization of Digges's programme as applied to a projectile's entire trajectory instead of just to its middle part.[18] But Digges's programme does not fully determine the geometrical shape of a projectile's trajectory. The laws that govern the two components of its motion also have to be specified, which was what they achieved by appealing to their inclined-plane analogy, in which the natural component is described by the law of free fall and the violent component by upward motion along an inclined plane. This analogy thus enabled a projectile's trajectory to be derived point by point from assumptions that were rooted in Aristotelian dynamics and that were specified by a mechanical device.

Galileo related a projectile's trajectory to yet another device found in early modern mechanics – a hanging chain (see Figure 5.10). He argued that the composite natural and violent motions of a projectile result in a trajectory of the same shape as that of a hanging chain (as viewed upside down).[19] His dynamical reasoning rested on his claim that the shape of a hanging chain results from the natural and violent forces acting on the chain, the natural force being produced by its weight and the violent force by its stretching at its points of suspension.

This analogy is at odds with classical mechanics. Galileo's dynamical reasoning is invalid; the shape of a hanging chain is not a parabola but a different mathematical curve, a hyperbolic cosine, which was unknown to Galileo and his contemporaries. Flawed as his analogy was, however, it nevertheless strongly supported his conviction that a projectile's trajectory is parabolic and symmetric about a vertical line running through its culmination point. As a consequence, he was unable to integrate his inclined-plane analogy into his theory of projectile motion, and this analogy remained fairly unexplored in his work.

Harriot's case was quite different. He did not relate a projectile's trajectory to the shape of a hanging chain and he did not demand that it be symmetrical. He therefore explored the inclined-plane analogy for projectile motion much further than Galileo; in fact, he elaborated this analogy in around 70 interrelated folio pages in his manuscripts. Making use of his algebraic formalism, he derived a formula for a projectile's time of flight; he calculated its range for various angles of projection; he determined the angle of projection for which its range is a

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[18] One may think that this reflects a fundamental insight concerning projectile motion, in that in Harriot's and Galileo's later theories the trajectory does not begin in a straight line but is curved. However, some evidence suggests that the straight line in the first part of the trajectory was regarded as an approximation by most early modern authors, including Tartaglia. It follows that the improvement represented by a curved line at the beginning of the trajectory appears to be due to an advanced means of geometrical construction rather than to a fundamental insight into the motion of a projectile.

[19] J. Renn, P. Damerow, S. Rieger and D. Giulini, 'Hunting the white elephant: when and how did Galileo discover the law of fall?', in J. Renn (ed.), *Galileo in context* (Cambridge, 2001), pp. 29–149, esp. pp. 35–40.

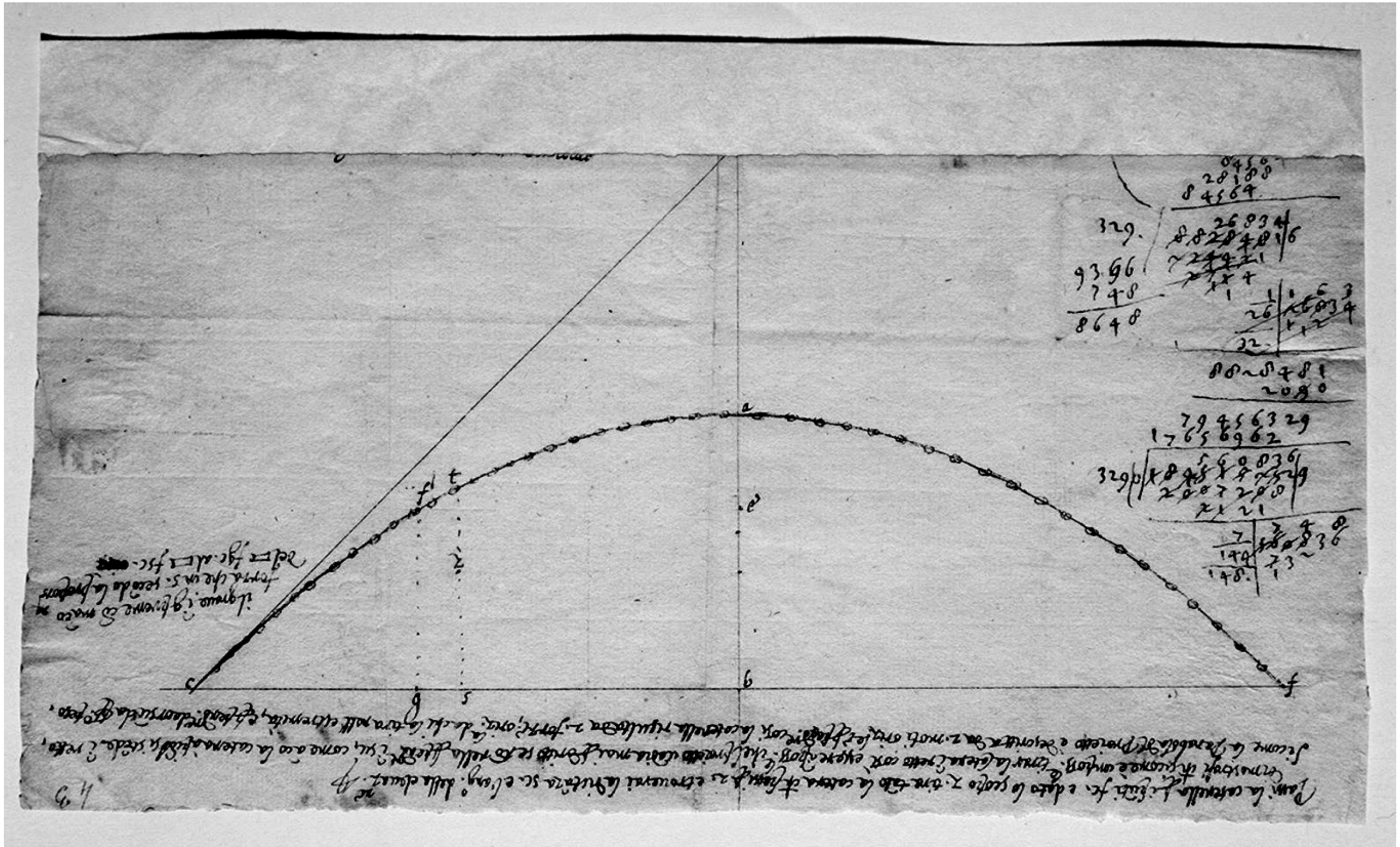

Figure 5.10 Galileo's comparison of the shape of a projectile trajectory with that of a hanging chain. Biblioteca Nazionale Centrale di Firenze, Galileo MS 72, f. 43. By concession of the Ministero per i Beni e le Attività Culturali della Repubblica Italiana/Biblioteca Nazionale Centrale di Firenze

maximum; he compared his theoretical ranges to empirical ranges that he found in the contemporary literature; and he gave a strict mathematical proof that the shape of a projectile's trajectory that followed from the inclined-plane analogy is indeed parabolic. For oblique projection, its trajectory turns out to be a tilted parabola, while for horizontal projection, it turns out to be an upright parabola.[20]

Individual pathways through the shared knowledge

Finally, I shall discuss differences between Harriot's and Galileo's inferential pathways that distinguish crucial results in their work. In so doing, I shall examine a further point of contact with classical mechanics, namely, the law of free fall, which both Harriot and Galileo formulated.

The law of free fall is a relationship between the space traversed by a freely falling body, initially at rest, and the time elapsed; it states that the former increases with the square of the latter. As such, the law is closely related to the parabolic shape of a projectile's trajectory, which, as we have seen, may be composed of a uniform motion in the horizontal direction and an accelerated motion in the vertical direction following the law of free fall. This, in fact, was the line of reasoning by which Galileo must have arrived at the law of free fall.[21] As such, his insight into the parabolic shape of a projectile's trajectory preceded his insight into the law of free fall.

Galileo, however, was not satisfied with only stating the law of free fall; he also sought to derive it. In a letter of 1604 that contains his first extant formulation of the law of free fall, he wrote that he was searching for a 'completely indubitable principle to put as an axiom' from which to derive it.[22] In the same letter, he also claimed to have found this principle, namely, the assumption that the velocity[23] of a falling body increases in direct proportion to the space traversed. Only later did he discover inconsistencies between this assumption and the law of free fall, and his considerations also showed him that these inconsistencies could be avoided by making a different assumption, namely, that the velocity increases in direct proportion to the time elapsed rather than to the space traversed – an assumption that is also correct in classical mechanics.

Galileo tried to derive the law of free fall from this assumption by using a medieval technique of graphical representation of motion that is also found in the work of Harriot and other contemporaries. But because Galileo encountered

20 Schemmel, *The English Galileo*, pp. 175–227.

21 Renn et al., 'Hunting the white elephant', pp. 51–3 and 127–8.

22 Galileo to Paolo Sarpi, 16 October 1604, in Galilei, *Le opere*, vol. 10 (Florence, 1968), pp. 115–16.

23 The term 'velocity' as used here does not refer to a vector quantity as is the case in modern physics. It is preferred here to the term 'speed' since it is closer to early modern terminology.

problems in relating the concept of velocity to this graphical representation, his attempt to derive the law of free fall failed, even though it was supported by a number of theorems and probably also by experimental results.[24] Galileo's inferential pathway may therefore be summarized schematically as follows:

> parabolic shape of a projectile's trajectory + decomposition of motions → law of free fall;
>
> law of free fall + additional theorem → velocity–time proportionality.

Let us now compare Harriot's inferential pathway with Galileo's. Harriot carried out experiments to decide whether the velocity of a freely falling body increases in direct proportion to the time elapsed or to the space traversed, these being the only two theoretical possibilities he considered. He dropped bullets from various heights onto a plate attached to one arm of a balance, as shown in Figure 5.11, which depicts my replication of Harriot's experiments. Harriot placed a certain counterweight in the scale-pan on the other arm of the balance and locked the beam of the balance in such a way that the arm bearing the counterweight was free to rise but could not descend (note the arresting wire attached to that arm of the balance). He then dropped the bullet from a certain height and observed if its percussive force on the plate could lift the counterweight. If it could not, he decreased the counterweight and tried again. In this way, he determined the largest counterweight that the bullet was able to lift when it hit the plate. He called this counterweight the 'weight' of the bullet after having fallen a certain distance and took it to be a measure of the bullet's velocity.

Harriot was not the only one to use a balance in free-fall experiments. Galileo also attempted to determine the force of percussion using a balance, albeit without much success, as he described in a text that remained unpublished during his lifetime and that was later included in some editions of the *Discorsi*.[25] The French Jesuit Pierre Le Cazre (1589–1664) envisioned a similar experiment and described it in his *Physica demonstratio* of 1645. In response to this, Pierre Gessendi (1592–1655) performed the experiment.[26] Marin Mersenne (1588–1648) also reported the experiment in 1647, which Willem Jakob 'sGravesande (1688–1742) mentioned a century later in his book *Physices elementa mathematica, experimentis confirmata*

[24] T.B. Settle, 'An experiment in the history of science', *Science*, 133 (1961), 19–23.

[25] Galilei, *Le opere*, vol. 8 (Florence, 1968), pp. 323–5.

[26] An account of the controversy about the motion of fall between Le Cazre and Gassendi and the role of the percussion experiment in it is given by C.R. Palmerino, 'Two Jesuit responses to Galileo's science of motion: Honoré Fabri and Pierre Le Cazre', in M. Feingold (ed.), *The new science and Jesuit science. Seventeenth century perspectives* (Dordrecht, 2003), pp. 187–227.

Figure 5.11 Author’s replication of Harriot’s percussion experiment

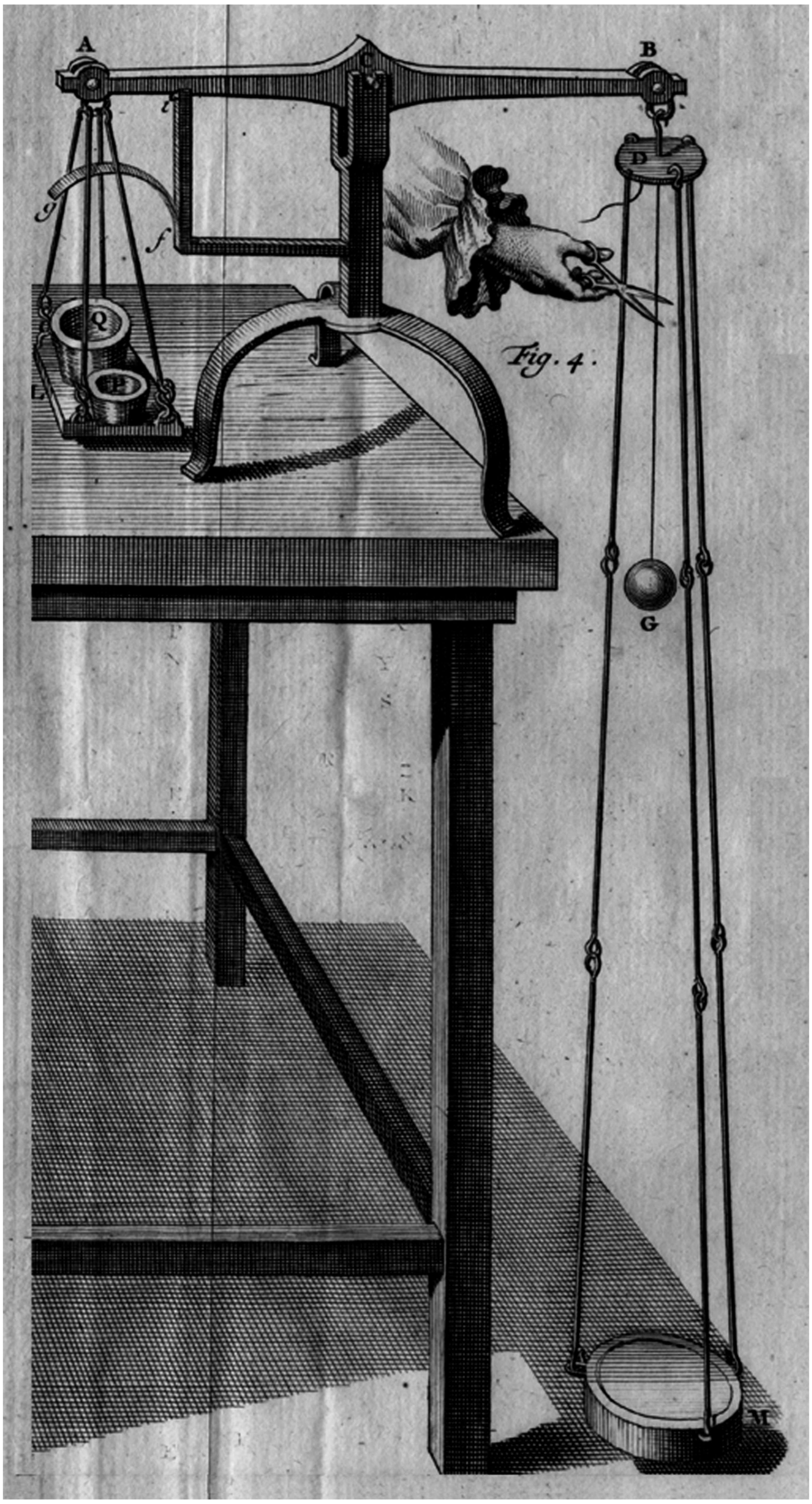

Figure 5.12 Willem Jakob ’sGravesande’s free-fall apparatus. The ball is suspended from a cord at a given height. The cord is then cut to ensure that the ball falls from rest, and the ball then strikes the plate attached to the right arm of the balance. Counterweights are placed on the plate attached to the arrested left arm of the balance. From ’sGravesande, *Physices elementa mathematica*, fold-out plate

of 1742,[27] in which Figure 5.12 originally appears. The use of a balance to measure the force of percussion thus appears to have been a rather common idea in early modern mechanics.

Let us now examine the inferential pathway in Harriot's work. Even though his understanding of his experiment is invalid in classical mechanics, he nevertheless was able to draw the correct conclusion from it: the velocity of a freely falling body is indeed proportional to the time elapsed. Harriot was more successful than Galileo in consistently relating the concept of velocity to the graphical representation of motion that both men were using. With the help of this representation, he was able to derive the law of free fall from the velocity-time proportionality and he applied it in his constructions of projectile trajectories, deriving their parabolic shape. As such, Harriot's inferential pathway may be summarized schematically as follows:

> velocity-time proportionality → law of free fall;
>
> law of free fall + composition of motions → parabolic shape of a projectile's trajectory.

Harriot's inferential pathway therefore proceeded in exactly the opposite direction to Galileo's. Despite this difference, however, both arrived at the law of free fall. Since both Harriot's and Galileo's pathways clearly were not based on classical mechanics, this demonstrates that the shared knowledge of the period fostered certain results in a way that was – to a certain degree – independent of the individual research pathways that were followed.

Conclusions

To return to our original questions: do the peculiarities of an individual scientist's work lead to diverging developments in science? Would we have a completely different physics today had there been no Galileo? Or do the lines of alternative development in science converge in such a way that the long-term development of science is not affected by local deviations owing to the peculiarities of an individual scientist's work? Such far-reaching questions, of course, cannot be answered based upon the analysis of the work of only two individuals: Harriot and Galileo. However, a comparison of their work and its fate should make a contribution.

We have seen that both Harriot's and Galileo's work contain insights that are valid in classical mechanics and became cornerstones in its development. I have termed these insights 'points of contact' with classical mechanics. But it is also clear that their work cannot be considered to be a part of classical mechanics.

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[27] W.J. 'sGravesande, *Physices elementa mathematica, experimentis confirmata. Sive, introductio ad philosophiam Newtonianam*, 3rd edn (Leyden, 1742).

In Galileo's case, his disciples and later scientists reinterpreted the theoretical framework in which his results were obtained. Hence, after the parabolic shape of a projectile's trajectory had been established, the motions comprising it could be inferred. In this way, Galileo's disciple Evangelista Torricelli (1608–47), for instance, concluded that a projectile's motion along its line of projection is uniform.[28] Such a process also seems possible in principle for Harriot's work. Despite the differences between Galileo and Harriot with respect to their objects of study, their experiments and their inferential pathways, their work nonetheless displays many common points of contact with classical mechanics. Harriot's case therefore suggests a convergence of the lines of development in early modern mechanics.

Yet Harriot failed to establish any connection to future generations of scientists. Owing to the restricted availability of his theoretical constructs to others, they effectively vanished from the subsequent history of science. This is in stark contrast to Galileo, who strenuously promoted his 'new sciences'. Galileo's theoretical constructs therefore were assimilated into the emerging knowledge structures of classical mechanics and eventually became part of a new science.

[28] Damerow et. al., *Exploring the limits of preclassical mechanics*, op. cit. (note 3), pp. 284–6. For a broader discussion of this mechanism of conceptual change, see J. Renn, 'Einstein as a disciple of Galileo: a comparative study of concept development in physics', *Science in context*, 6 (1993), 311–41, esp. 320–23.

Chapter 6

Why Thomas Harriot Was *Not* the English Galileo

John Henry

Beginning with Baron von Zach (1754–1832) in the eighteenth century, Thomas Harriot has commonly been seen as an English Galileo. Reaffirmed in the late twentieth century by Jean Jacquot and other devotees of Harriot, this claim has most recently been defended by Matthias Schemmel.[1] It is important to note, before going any further, that in many respects this claim is entirely justifiable. The title of 'English Galileo' is based on Harriot's independent discovery of the parabolic trajectory of projectiles; studies of impact and force of percussion or collision which approached closer to the classical solution than Galileo managed; and the independent manufacture and use of telescopes to observe the surface of the moon and the phenomena of sunspots before Galileo.[2] Like Galileo, Harriot also tried to develop an atomistic theory of matter, although for different reasons: in Galileo's case, atomism was used chiefly as a means of explaining cohesion and the difference between liquid and solid states, while Harriot seems to have

[1] F. Xaver von Zach, an Austrian mathematician and astronomer, rediscovered Harriot's papers during a visit to Petworth House in 1784. For a full account of his attempts to establish Harriot as the English Galileo, see J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), pp. 13–23. See also J. Jacquot, 'Harriot, Hill, Warner and the new philosophy', in J.W. Shirley (ed.), *Thomas Harriot, Renaissance scientist* (Oxford, 1974), pp. 107–28 (p. 107). M. Schemmel, 'Thomas Harriot as an English Galileo: the force of shared knowledge in early modern mechanics', *Bulletin of the Society for Renaissance Studies*, 21 (2003), 1–9; expanded in 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', *Physics in perspective*, 8 (2006), 360–80. See also M. Schemmel, *The English Galileo. Thomas Harriot's work on motion as an example of preclassical mechanics*, 2 vols (London, 2008).

[2] The literature on Galileo is vast, but for an account of his work, see S. Drake, *Galileo at work. His scientific biography* (Chicago, 1978). For discussions of the similarities between Harriot and Galileo, see Shirley, *Thomas Harriot. A biography*; the works by Schemmel cited in the previous note; and J.D. North, 'Thomas Harriot and the first telescopic observations of sunspots', in Shirley (ed.), *Thomas Harriot, Renaissance scientist*, pp. 129–65.

regarded atomism as the best system to explain condensation and rarefaction, specific weights and optical refraction.[3]

Furthermore, Harriot can also be compared with other great figures in the history of science. Take René Descartes, for example. Descartes has a claim to being the independent discoverer in optics of the law of refraction which is now known as Snell's law (after Willebrord Snell), but Harriot beat both Snell and Descartes to it.[4] It is also arguable that Harriot, not Descartes, should be credited with the invention of the algebraic technique of shifting all the terms of an equation to the left-hand side and making them equal to zero.[5] Evidence that Harriot denied the principle of celestial circularity and dared to believe that planets might not move in perfect circles, even before the appearance of Johannes Kepler's *Astronomia nova* in 1609, has led to an honourable mention as the English Kepler. In what follows, therefore, we will occasionally compare Harriot to Descartes and Kepler, as well as to Galileo.[6]

However, in spite of the undeniable similarities between the work of Harriot and these most eminent of his contemporaries in the history of science, I want to reject suggestions that he was, to stick to my title for now, an English Galileo. Nevertheless, it is important to state at the outset that this chapter is not concerned to deny, much less refute, all earlier claims that have pointed to the undeniable similarities between the work and intellectual achievements of Galileo and Harriot. Therefore, this is not intended to be a refutation of Dr Schemmel's work, for example. Schemmel has concentrated on Harriot's mathematical work and its similarities to Galileo's mathematical work; the validity and the value of this work are not in dispute in what follows. This chapter merely seeks to make a more general historical point, which has important historiographical implications, about

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[3] J. Henry, 'Thomas Harriot and atomism: a reappraisal', *History of science*, 20 (1982), 267–96.

[4] J.W. Shirley, 'An early experimental determination of Snell's law', *American journal of physics*, 19 (1951), 507–8; and *Thomas Harriot. A biography*, pp. 380–88. On Descartes's discovery of the law of refraction, see J.A. Schuster, 'Descartes *opticien*: the construction of the law of refraction and the manufacture of its physical rationales, 1618–29', in S. Gaukroger, J. Schuster and J. Sutton (eds), *Descartes' natural philosophy* (London and New York, 2000), pp. 258–312.

[5] So Charles Cavendish is supposed to have claimed to Roberval, as reported by John Wallis. See J. Stedall, *The greate invention of algebra. Thomas Harriot's Treatise on equations* (Oxford, 2003), pp. 28–9. See also M. Seltman, 'Harriot's algebra: reputation and reality', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 153–85.

[6] Indeed, Zach likened Harriot to Kepler as well as to Galileo; see Shirley, *Thomas Harriot. A biography*, p. 16. Harriot has also been likened to the Danish astronomer, alchemist and natural philosopher Tycho Brahe; see J.A. Lohne, 'Thomas Harriot, 1560–1621: the Tycho Brahe of optics', *Centaurus*, 6 (1959), 113–21.

the considerable differences between Harriot and Galileo.[7] In part, my aim in exposing these crucial differences is to explain why, for all his astonishing genius, Harriot failed, in the judgement of history, to scale the heights that Galileo did. As such, my denial that Harriot was an English Galileo should not be seen as an attempt to belittle this great English thinker, but as an attempt to explain why so often he did not capitalize on his innovations and achievements.

Before I give my own reasons for denying that Harriot was an alternative Galileo, it is perhaps worth pointing to another denial based on very different grounds. Stephen Pumfrey has observed the very real differences in the situations of Galileo and Harriot as a result of the different styles of patronage they received.[8] These differences were so great, he argues, that it had a very real bearing on the final achievement of both men and resulted in Harriot being Harriot, and Galileo, and only Galileo, being Galileo.

Although Galileo began his career as a professor of mathematics at the University of Pisa and subsequently at Padua, after the publication of his sensational *Sidereus nuncius*, he seized the opportunity to ingratiate himself with Cosimo II, Grand Duke of Tuscany, and remained a leading figure in Cosimo's court until his fall from grace in 1633. His role at court was to advance Cosimo and the Medici in their cultural rivalry with the courts of neighbouring city states, including the Papal court in Rome. Accordingly, he had to be ostentatious, producing work that would make a big noise through the republic of letters, making his own name and simultaneously enhancing that of his patron.[9]

It was very different in Elizabethan England. Once Harriot left the employ of his first patron, Sir Walter Ralegh, whose demands on Harriot's expertise were almost entirely pragmatic, and became, from about 1595, the pensioner of Henry Percy, the ninth Earl of Northumberland, there was perhaps some expectation that he would enhance the reputation of the Wizard Earl, but it was clearly a far cry from the demands on Galileo as courtier.[10] For one thing, Henry Percy was a courtier himself, serving his monarch, and was not involved in the same kind of rivalry with fellow courtiers as Cosimo was with neighbouring princes. Percy was involved in a rivalry for the monarch's favour, not for his own aggrandizement. Furthermore,

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[7] Matthias Schemmel does not need to address the differences that are the focus of this chapter and does not do so. His approach remains entirely valid, however. See the works cited in note 1 above.

[8] S. Pumfrey, 'Was Harriot the English Galileo? An answer from patronage studies', *Bulletin of the Society for Renaissance Studies*, 21 (2003), 11–22.

[9] Ibid.; and S. Pumfrey and F. Dawbarn, 'Science and patronage in England, 1570–1625', *History of science*, 42 (2004), 137–88. See also Pumfrey's chapter in this volume; M. Biagioli, *Galileo, courtier. The practice of science in the culture of absolutism* (Chicago, 1993); and M. Biagioli, *Galileo's instruments of credit. Telescopes, images, secrecy* (Chicago, 2006).

[10] Details of Harriot's biography can be found, of course, in Shirley, *Thomas Harriot. A biography*.

it followed from their markedly different positions in their respective political hierarchies that Cosimo and Percy did not have the same motivation for supporting these two mathematicians. Unlike Cosimo, Percy was genuinely interested in the work of his so-called three magi: Harriot, Walter Warner and Robert Hues. He shared their passion for an understanding of the natural world. Again, this meant that Harriot's work did not need to be played out so publicly as Galileo's. Cosimo had no use for a hermit-like scholar, burning his light under a bushel, whereas Percy seems to have felt well satisfied by a few days spent with Warner or Harriot performing a fruitless series of alchemical manipulations or trying to learn how to solve various problems using the latest algebraic techniques.[11] Galileo had to produce work that would have a big impact, sending out shock waves through the republic of letters. Harriot, who already enjoyed the intellectual engagement of his patron, could afford to be 'contented with a private life for the love of learning that I might study freely'.[12]

Another difference between the situations of Galileo and Harriot hinged upon what Pumfrey calls connectivity. It would have been an easy matter for Galileo to find another patron, either in a neighbouring Italian state or perhaps at the Habsburg court in Prague. This meant that a man like Galileo could maintain some leverage over his patron, ensuring at least some means of maintaining conditions at court that suited him. In England there were few opportunities, and a man like Harriot, who was generously retained, had to consider himself lucky. Even John Dee, after all, never found a similar permanent position in England. He was commissioned by Elizabeth I for various tasks, but never succeeded in persuading her to make him her court magus. His only recourse was to take up offers of patronage on the Continent – a move which then made him suspect back in England. Harriot did try to find favour with other possible patrons, notably Robert Cecil, first Earl of Salisbury, and even the young Prince of Wales, Henry, but they died before they could do him any real good. What this meant was that Harriot was never able to break free of the somewhat baleful influence upon him of the reputations of his two successive patrons Ralegh and Percy. The historical jury is still out, pondering whether Harriot was an atheist or not, but the contemporary jury found him guilty. A substantial portion of his guilt seems to have been acquired by association. If his reputation for impiety, or for political chicanery, were not of his own making, he could do nothing to dissociate himself from those who were to blame, because

11 Shirley, *Thomas Harriot. A biography*, which can be supplemented by his 'The scientific experiments of Sir Walter Ralegh, the Wizard Earl, and the three magi in the Tower, 1603–1617', *Ambix*, 4 (1949–51), 52–66; and 'Sir Walter Raleigh and Thomas Harriot', in Shirley (ed.), *Thomas Harriot. Renaissance scientist*, pp. 16–35. See also R.H. Kargon, *Atomism in England from Hariot to Newton* (Oxford, 1966); and Jacquot, 'Harriot, Hill, Warner and the new philosophy'.

12 Pumfrey, 'Was Harriot the English Galileo?', p. 16, quoting from Shirley, *Thomas Harriot. A biography*, p. 349.

of the difficulties of finding patrons in England.[13] In that sense, therefore, he was in a worse position than Galileo, whose troubles with the Church *were* largely of his own making.[14]

Thus, there can be no doubt that differences in the nature of their patronage, stemming either from the socio-political differences in the nature of patronage in early seventeenth-century Tuscany and England or from the personal differences and demands of their respective patrons, ensured that Harriot and Galileo marched to the beat of different drums, and this very much affected their output of work. As such, I entirely endorse Pumfrey's claim that our understanding of the nature of patronage as it affected the careers of both men is extremely important and helps us to see why, in spite of so many might-have-beens, Harriot did not achieve what Galileo did, and did *not* become the English Galileo. Or, rather, perhaps we should say that Harriot's failure to rush his telescopic observations of the moon or of sunspots into print meant that Galileo is not now routinely described as the Italian Harriot.

However, I believe that there is yet more to be said on this matter, for there is another significant reason why Harriot cannot seriously be regarded as an English Galileo. And it is this aspect of Harriot and his work that I want to focus on for the rest of this chapter. I can bring to the fore just what this is by reminding the reader of a famous aspect of Galileo's negotiations with Belisario Vinta, Cosimo II's Secretary of State, when he first arranged the terms of his appointment to the Ducal Court. 'As to the title of my position', Galileo wrote:

> I desire that in addition to the title of 'mathematician' His Highness will annex that of 'philosopher'; for I may claim to have studied more years in [natural] philosophy than months in pure mathematics.[15]

And so it was that Galileo became mathematician *and philosopher* to the Grand Duke of Tuscany. Galileo lived up to this title in a way that no contemporary could have foreseen. He was, of course, a leading figure in the transformation of natural philosophy into something much more recognizably like modern science, by amalgamating speculative natural philosophy with mathematical and experimental traditions. Harriot, though, always remained first and foremost a mathematical practitioner. While Galileo clearly wanted to be seen as a natural philosopher, the evidence suggests that Harriot always remained reluctant to venture into natural

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13 See E.A. Strathmann, 'The History of the World and Raleigh's scepticism', *Huntington Library quarterly*, 3 (1940), 265–87; and J. Jacquot, 'Thomas Harriot's reputation for impiety', *Notes and records of the Royal Society*, 9 (1952), 164–87.

14 On Galileo's tactless handling of the concerns of contemporary Catholic theologians, see, for example, E. McMullin (ed.), *The Church and Galileo* (Indiana, 2005).

15 G. Galilei, letter to Belisario Vinta, May 1610. For a full discussion of Galileo's negotiations, see Biagioli, *Galileo, courtier*.

philosophy and therefore, I submit, was unable to participate in its transformation into a new form of philosophy.

So what I must do now is to explain what I mean by this transformation of natural philosophy, in the course of which it was amalgamated with mathematical and experimental traditions. Only when we have a clear idea of the shifting boundaries in the organization of knowledge in Harriot's day can we see where his work was located. I hope then that I will be able to persuade the reader finally that Harriot could not have been the English Galileo, because Galileo was a natural philosopher, albeit of a new breed, but Harriot never was.

The separation between mathematics and natural philosophy in Harriot's day

There was a clear distinction still in force in the sixteenth century between natural philosophy, sometimes simply called physics or *physiologia*, and the mathematical sciences. According to the Aristotelian precepts, natural philosophy was concerned with understanding the 'why' of things, and this required a knowledge of causes, how these things came to be as they are. Thus, natural philosophy was concerned with causal narratives that not only identified the causes but also showed how the causes operated to bring about the state of affairs in question. Implicit, if not explicit, in this starting point was the assumption that natural philosophy dealt with 'sensible being', material bodies as they appeared to the senses, and this in turn marked natural philosophy out from mathematics, which considered 'quantitative being', and metaphysics, which dealt with 'being in itself', abstracted from any substantial instances of being.[16]

Generally speaking, mathematics was seen as an ineffective way of pursuing natural philosophical enquiry. Mathematics could not provide the required causal accounts, but could only provide what amounted to a special kind of precisely detailed description. Analysis of the motion of the sphere of Mars, for example, could reveal that it must move on a large epicycle around a deferent and could determine the required speeds of motion on both epicycle and deferent, the amount of eccentricity of the deferent relative to the earth and so on. But it could not offer any account of why Mars moved this way – of what caused these complicated movements.[17]

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[16] See N. Jardine, 'Epistemology of the sciences', in C.B. Schmitt and Q. Skinner (eds), *Cambridge history of Renaissance philosophy* (Cambridge, 1988), pp. 685–711; J. Høyrup, 'Philosophy: accident, epiphenomenon, or contributory cause of the changing trends of mathematics – a sketch of the development from the twelfth through the sixteenth century', in *In measure, number, and weight. Studies in mathematics and culture* (Albany, 1994), pp. 123–71; P. Dear, *Discipline and experience. The mathematical way in the scientific revolution* (Chicago, 1995); and P. Mancosu, *Philosophy of mathematics and mathematical practice in the seventeenth century* (Oxford, 1996).

[17] See, for example, Jardine, 'Epistemology of the sciences', pp. 697–702.

Nevertheless, it was recognized that some mathematical sciences came close to the domain of natural philosophy insofar as they dealt with real physical phenomena. Astronomy was one of these, as was music (the science of mathematical ratios, which clearly corresponded in some way to the physical harmonies heard in music) and optics, the geometrical study of the behaviour of light rays. These were designated in the Aristotelian tradition as *scientiae mediae*, or mixed sciences, because they used the principles of mathematics to understand what was taking place, but were part of physics because they dealt with sensible things. The mixed sciences were said to be subalternated to mathematics on the one hand and to physics on the other, and there were endless scholastic debates as to which of the parent sciences dominated.[18]

Now, by the second half of the sixteenth century change was already taking place, and the mixed sciences were beginning to play a greater role in natural philosophy. In particular, a new *scientia media*, mechanics, was beginning to be included in discussions of motion in natural philosophy. Mechanics had previously been regarded as an art rather than as one of the mathematical sciences, because its purpose was entirely practical. But the Renaissance rediscovery of the 'Mechanical problems', attributed to Aristotle (though admittedly never uncontroversially accepted as genuine), made the learned aware of the theoretical credentials of mechanics, and it was accordingly placed alongside the other mixed mathematical sciences. Although mechanics dealt chiefly with processes taking place in artificial set-ups, such as machines, and should therefore have been excluded from natural philosophy, which was supposed to deal with unforced, perfectly natural processes, it was occasionally introduced alongside discussions of projectile motion, which Aristotle had included in the *Physics*.[19]

The increasing attention paid to the mathematical sciences in natural philosophy was due in no small measure to the efforts of a very wide range of different mathematicians to increase the intellectual status of their subject. There were

[18] See, for example, W.A. Wallace, 'Traditional natural philosophy', in Schmitt and Skinner (eds), *The Cambridge history of Renaissance philosophy*, pp. 201–35; S.J. Livesey, 'Science and theology in the fourteenth century: the subalternate sciences in Oxford commentaries on the *Sentences*', *Synthèse*, 83 (1990), 273–92; and P. Harrison, 'Physico-theology and the mixed sciences: the role of theology in early modern natural philosophy', in P.R. Anstey and J.A. Schuster (eds), *The science of nature in the seventeenth century. Patterns of change in early modern natural philosophy* (Dordrecht, 2005), pp. 165–83.

[19] For more detailed discussions, see A. Gabbey, 'Newton's *Mathematical principles of natural philosophy:* a treatise of mechanics?', in P. Harman and A. Shapiro (eds), *The investigation of difficult things. Essays on Newton and the history of the exact sciences* (Cambridge, 1992), pp. 305–22; A. Gabbey, 'Between *ars* and *philosophia naturalis*: reflections on the historiography of early modern mechanics', in J.V. Field and F.A.J.L. James (eds), *Renaissance and revolution. Humanists, scholars, craftsmen and natural philosophers in early modern Europe* (Cambridge, 1993), pp. 133–45; and H. Hattab, 'From mechanics to mechanism: the *Quaestiones mechanicae* and Descartes' physics', in Anstey and Schuster (eds), *The science of nature*, pp. 99–129.

mathematical humanists, recovering ancient mathematics, university mathematics teachers and a wide range of mathematical practitioners, from elite architects and military engineers to more humble teachers and artisans, all of whom were trying to make a living from mathematics and, in many cases, to exploit the interconnected possibilities for patronage that existed in continental Europe, if not in England.[20] Nevertheless, there was a great deal of inertia in scholastic natural philosophy, as in the neighbouring disciplines, so there was still a great deal of separation between natural philosophers and mathematical practitioners.[21]

The classic illustration of this, of course, is the preface added to Copernicus's *De revolutionibus orbium coelestium* by the Lutheran minister who had been delegated to supervise it through the press, Andreas Osiander. Judging from the opening remark, Osiander had heard of objections to Copernicus's approach to astronomy even before it was published and sought to forestall them.[22] Accordingly, and without asking for the author's permission, he interpolated a page, before Copernicus's own preface, that bore only his own unsigned preface:

> There have already been widespread reports about the novel hypotheses of this work, which declares that the earth moves whereas the sun is at rest in the center of the universe. Hence certain scholars, I have no doubt, are deeply offended and believe that the liberal arts, which were established long ago on a sound basis, should not be thrown into confusion. But if these men are willing to examine the matter closely, they will find that the author of this work has done nothing blameworthy. For it is the duty of an astronomer to compose the history of the celestial motions through careful and expert study. Then he must conceive and devise the causes of these motions or hypotheses about them. Since he cannot

20 P.L. Rose, *The Italian renaissance of mathematics. Studies on humanists and mathematicians from Petrarch to Galileo* (Geneva, 1975); M. Biagioli, 'The social status of Italian mathematicians, 1450–1600', *History of science*, 27 (1989), 41–95; W.R. Laird, 'Patronage of mechanics and theories of impact in sixteenth-century Italy', in B.T. Moran (ed.), *Patronage and institutions. Science, technology, and medicine at the European Court, 1500–1750* (Woodbridge, 1991), pp. 51–66; J. Bennett, 'The mechanical arts', in K. Park and L. Daston (eds), *The Cambridge history of modern science. Volume 3. Early modern science* (Cambridge, 2006), pp. 673–95.

21 For a fuller discussion of the prolonged resistance by natural philosophers to what has been called 'the mathematization of the world picture' (see note 59 below), see J. Henry, 'The origins of the experimental method – mathematics or magic?', in H. Busche and S. Hessbrueggen-Walter (eds), *Departure for modern Europe. Philosophy between 1400 and 1700* (Hamburg, 2010), pp. 702–14.

22 Presumably circulating after the appearance of G. Rheticus, *De libris revolutionum … D. Doctoris Nicolai Copernici…, narratio prima* (Basel, 1541). On the background to the publication of *De revolutionibus* and Osiander's role in it, see A. Koyré, *The astronomical revolution* (London, 1973), pp. 34–42; and B. Wrightsman, 'Andreas Osiander's contribution to the Copernican achievement', in R.S. Westman (ed.), *The Copernican achievement* (Los Angeles, 1975), pp. 213–43.

> in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly from the principles of geometry for the future as well as for the past. The present author has performed both these duties excellently … For this art, it is quite clear, is completely and absolutely ignorant of the causes of the apparent non-uniform motions. And if any causes are devised by the imagination, as indeed very many are, they are not put forward to convince anyone that they are true, but merely to provide a reliable basis for computation …
>
> So far as hypotheses are concerned, let no one expect anything certain from astronomy, which cannot furnish it, lest he accepts as the truth ideas conceived for another purpose, and depart from this study a greater fool than when he entered it. Farewell.[23]

As Robert S. Westman has pointed out, it is not the nature of the physical world that is in danger of being thrown into confusion by Copernicus's heliocentric astronomy, but the liberal arts, to which astronomy has long since been allocated. To reassure his readers, Osiander immediately reaffirms that the author is not saying anything about true causes – that is to say, he is not making any natural philosophical claims – but is merely considering hypothetical, we might say simply 'mathematical', constructs which enable us to correctly calculate planetary positions. Copernicus himself undoubtedly wanted to assert a new relationship between mathematics and natural philosophy, but Osiander has undercut his intentions by reaffirming the traditional boundary between natural philosophy and the subalternate mixed science of astronomy. 'The upshot of Osiander's skilfully argued *Letter* is striking', Westman wrote, 'in believing that he has demonstrated the astronomer's *inability* to draw conclusions in natural philosophy, he denies him the *right* to do so.'[24]

That the gulf between natural philosophy and mathematics remained wide even for the succeeding generation of Copernican astronomers can be seen in the fact that the prime movers in establishing the truth of the Copernican astronomy (prime movers at least in the judgement of history) all turn to causal explanations as to how the earth moves and do not assume that the mathematics can speak for itself. In this they differ from Copernicus himself, who famously wrote that mathematics is written for mathematicians.[25] Effectively, Copernicus simply hoped that what might be called the mathematical aesthetics of his system would carry the day. He hoped that the fact that his system provided a unique and fixed

[23] [Andreas Osiander], 'To the Reader concerning the hypotheses of this work', in N. Copernicus, *On the revolutions*, trans. E. Rosen (Baltimore, MD, 1992), p. xx.

[24] R.S. Westman, 'The astronomers' role in the sixteenth century: a preliminary survey', *History of science*, 18 (1980), 105–47 (108–9).

[25] Copernicus, 'To His Holiness, Pope Paul III', in *On the revolutions*, p. 5 (although Rosen translates the famous phrase 'Mathemata mathematicis scribuntur' as 'Astronomy is written for astronomers').

order of the planets which was entirely compatible with the orbital periods of the planets (unlike the Ptolemaic system, in which the order of the planets was only conventional) would persuade his readers of the physical truth of his system.[26] But such hopes were only realistic, and Copernicus knew it, if his readers were mathematicians; only they could see the point. Kepler was not content to speak only to mathematicians, however, and so the full title of his *Astronomia nova* announced that it was an astronomy based on causes (*astronomia aitiologetos*), or a celestial physics (*physica coelestis*).[27] Whether any non-mathematicians were tempted to take a look at Kepler's book remains doubtful, but Galileo moved almost entirely away from mathematics in his attempts to establish the truth of Copernican theory. He presented the observations he made with the telescope as powerful circumstantial evidence in favour of the theory, and in his 'Dialogue on the two chief world systems' he developed a whole new natural philosophical theory of motion to show how the earth *could be* in perpetual circular motion, and presented his explanation of the tides to argue that it *must be* in motion.[28]

This brings us, by way of contrast, to Harriot. When Harriot looked at the moon through the telescope he had made, he drew what he saw, just as Galileo was to do some months later. However, unlike Galileo, he did not set down in writing what he made of this; he did not discuss what general conclusions might be drawn from what he saw, much less tell us what conclusions, if any, he drew from his conclusions. Similarly, he meticulously recorded his observations of sunspots, but made no pronouncements upon them. His records indicate that he thought of them as spots on the surface of the sun. Harriot did not make the same mistake as the Jesuit astronomer Christoph Scheiner, who assumed they must indicate a planet (or planets) circling the sun inside the orbit of Mercury; evidently he could tell by the way the spots clustered, their sometimes ragged appearance, and the way they appeared and disappeared, that they were not planets.[29] But whether he ever gave any thought to the fact that these spots undermined the Aristotelian claim that the

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[26] See Koyré, *The astronomical revolution*, pp. 43–54.

[27] J. Kepler, *Astronomia nova ΑΙΤΙΟΛΟΓΗΤΟΣ [aitiologetos], seu physica coelestis, tradita commentariis de motibus stellæ Martis, ex observationibus G. V. Tychonis Brahe* (Prague, 1609). This has been translated by W.H. Donahue: J. Kepler, *New astronomy* (Cambridge, 1992).

[28] G. Galilei, *Sidereus nuncius, or the sidereal messenger* [1610], trans. A. Van Helden (Chicago, 1989); and *Dialogue concerning the two chief world systems – Ptolemaic and Copernican* [1632], trans. Stillman Drake (Berkeley, CA, 1953). The argument for the earth's motion based on the tides appears in the *Dialogue* on the fourth day.

[29] W.R. Shea, 'Scheiner, and the interpretation of sunspots', *Isis*, 61 (1970), 498–519; W.R. Shea, *Galileo's intellectual revolution* (Basingstoke, 1972), pp. 49–74; and J.D. Moss, *Novelties in the heavens. Rhetoric and science in the Copernican controversy* (Chicago, 1993), pp. 97–125. See also North, 'Thomas Harriot and the first telescopic observations of sunspots'; and S. Clucas, 'Thomas Harriot and the field of knowledge', in Fox (ed.) *Thomas Harriot*, pp. 93–136, esp. pp. 120–22.

heavens are perfect and unchanging, we simply do not know. Not only did he *not* write an English *Sidereus nuncius* or a *Letter on sunspots*, as far as we can tell he did not even make any private notes about these things to accompany his records of his observations. If he thought anything, he either kept those thoughts to himself or was content merely to discuss them with his immediate companions.[30]

It is almost as though Harriot had his own Osiander inside his head, a voice of conscience which told him that, as an astronomer he did not have the *ability* to draw conclusions in natural philosophy and therefore he had no *right* to try to do so.[31]

It may be useful to consider another illustration, which shows on the one hand the continuing gulf between mathematical and natural philosophical work and on the other the kind of innovatory work Harriot might have done, but did not. This illustration is based on the discovery of magnetic dip or declination, discovered by the retired mariner turned compass-maker Robert Norman and published by him in 1581 in his *Newe attractive*. Norman seems to have been aware of the phenomenon of declination for a while, but only paid attention to it when it caused him to ruin one of his compass needles. He had noted that a perfectly balanced iron needle would no longer remain horizontal (as though balanced) on its pivot after it had been magnetized by rubbing with a lodestone. The needle would now dip down towards the north as though it were heavier at that end. Routinely, he would shave off some iron from the north pole of the magnetized needle to restore it to the horizontal. He was moved to investigate this further when, on one occasion, he took too much weight off the end of a particularly large commissioned needle and so, 'stroken into some choler', had to start again with another piece of iron.

Norman took advice on how to proceed in his investigations from 'certaine learned and expert men':

> I applied my self to seeke further into this effect, and makyng certaine learned and expert men, my freendes, acquainted in this matter, they advised me to frame some Instrument, to make some exacte triall, how much the Needle touched with the Stone would Decline, or what greatest Angle it would make with the plane of the Horizon. Whereupon I made diligent proofes, the maner whereof is shewed in the Chapter followyng.[32]

Now it seems to me that these expert friends must have been mathematical practitioners rather than natural philosophers. I base this judgement not simply on the surely undeniable fact that Norman was much more likely to know mathematical practitioners of various stamps than he was to know any natural philosophers. I

[30] G. Galilei, *Sidereus nuncius*; and *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* (Rome, 1613).

[31] Westman, 'The astronomers' role', 108–9.

[32] R. Norman, *The newe attractive, containing a short discourse of the magnes or lodestone* (London, 1581), p. 9.

also base it on the nature of the trials he was advised to make. The emphasis is all upon measuring aspects of the needle's behaviour. Norman is not advised to make exploratory experiments, but 'exacte triall[s]'. He is advised to try to determine *by how much* the needle declines, not why it does so. When Norman followed this advice he was able to come up with a new navigational instrument – the dip circle – that would reveal a ship's latitude even under cloudy skies, when no heavenly bodies were visible. What he did not come up with was a natural philosophical explanation, in causal terms, of why this phenomenon took place.[33]

Just this kind of natural philosophical explanation for the phenomenon was provided in 1600 by William Gilbert in his famous work of experimental physics, *De magnete*. We do not know when Gilbert became aware of Norman's work, but at some point he seized upon it and made it the starting point of a new system of natural philosophy. Gilbert saw that the dip of the needle was due to the fact that the magnetic pole of the needle was pointing directly to the magnetic pole of the earth and not, as was previously held, to the pole star in the heavens. Pointing directly to the magnetic pole, the needle pointed down below the horizon, through the earth itself. He surmised that at the magnetic pole the needle would point straight down, vertically into the earth, while at the equator, the needle would hang horizontally on its pivot. He was then able to demonstrate that this was precisely what happened when a small magnetic needle was suspended above different parts of the surface of a spherical magnet (a *terrella* in Gilbert's terminology). It followed from this that the earth itself was a giant spherical magnet. From here, he was able to go on to use the magnetic nature of the earth to argue for its perpetual circular motions, as demanded by Copernican astronomy.[34]

It seems to me that Norman and Gilbert were two completely different kinds of thinker. Norman was concerned only with knowing how to put to pragmatic use the phenomenon that he accidentally discovered. His idea of investigating the phenomenon of magnetic declination seemed to consist merely in confirming that it was real and then in taking stock of its precise behaviour in order to see how it might be put to use; this enterprise was ultimately embodied in the fact that he invented a new instrument to measure the phenomenon. Gilbert, by contrast, seized on magnetic dip precisely because he saw how it could be used to support Copernican astronomy, by pointing the way towards a *causal* account of how the earth could maintain itself in perpetual motion. Gilbert's new natural philosophy was innovatory in its content but entirely traditional in its concern to offer causal explanations.[35]

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33 For a fuller discussion of this aspect of Norman's work, see Henry, 'The origins of the experimental method'.

34 For a fuller account, see J. Henry, 'Animism and empiricism: Copernican physics and the origins of W. Gilbert's experimental method', *Journal of the history of ideas*, 62 (2001), 99–119.

35 Ibid.

Comparisons are odious. But if we had to compare Harriot with one of these two magnetic experimenters, I believe that we should have to count him as closer to Norman than to Gilbert. Harriot's experiments, in whatever area, all seem to have been, like Norman's, in the nature of 'exacte triall[s]', to take measurements and to carefully record changes. There is no evidence that he ever did an experiment to test, or to demonstrate, a putative theoretical explanation of the nature of the world.

Harriot: mathematician but no natural philosopher

I suggest that it is Harriot's refusal to enter the natural philosophy stakes that lies behind the frustration and disappointment which scholars of Harriot often feel the need to express. Whether it be John Shirley commenting on his alchemy ('nowhere does he indicate why he is doing what he is doing, and nowhere does he give any conclusion as to what he has found or failed to find'), John North commenting on his observations on sunspots ('He left no explanations of the phenomena he so painstakingly recorded, and for this reason he simply cannot be compared in historical terms with Galileo') or Hilary Gatti commenting on other aspects of his work ('Harriot himself left no ordered and coherent body of philosophical speculation'), there is no shortage of expressions of frustration and exasperation with Harriot.[36]

However, as Jim Bennett has pointed out, the fault is ours, not Harriot's. Harriot could not have foreseen our historiographical preoccupations and may not have wished to pander to them if he had.[37] And yet there has been a strong tendency among Harriot scholars to assume that if he was not the English Galileo, he should have been! Jean Jacquot, for example, seemed to want to imply that Harriot did try to reform natural philosophy (like Galileo) but that evidence of his efforts has been lost:

> Considering the scope and originality of his research, one may wonder to what extent he felt the need of a new philosophy to replace the old, and to what extent he was able to shape its elements into a coherent pattern. Unfortunately the evidence to be found in his writings is fragmentary.[38]

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[36] Shirley, *Thomas Harriot. A biography*, p. 272; North, 'Thomas Harriot and the first telescopic observations of sunspots', p. 147; H. Gatti, 'The natural philosophy of Thomas Harriot', in Fox (ed.), *Thomas Harriot*, pp. 64–93, esp. p. 92.

[37] J. Bennett, 'Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning', in Fox (ed.), *Thomas Harriot*, pp. 137–52 (p. 139). See also Clucas, 'Thomas Harriot and the field of knowledge'.

[38] Jacquot, 'Harriot, Hill, Warner', p. 107.

This attitude is often held to have been summed up by Harriot's friend Sir William Lower:

> Do you not here startle, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it, in print. a little before Vieta prevented you of the Gharland for the great Invention of Algebra. al these were your dues and manie others that I could mention; and yet too great reservednesse hath robd you of these glories … Onlie let this remember you, that it is possible by too much procrastination to be prevented in the honour of some of your rarest inventions and speculations. Let your Countrie and friends injoye the comforts they would have in the true and great honour you would purchase your selfe by publishing some of your choise works.[39]

Harriot was writing in February 1610 and so it is all too easy to imagine Lower being startled once again soon after when Galileo's *Sidereus nuncius* hit the bookstands, and again when the *Letters on sunspots* appeared. Accordingly, there has been a tendency to read this passage as an implicit foreshadowing of the claim that Harriot could have been the English Galileo. But this is to read too much into Lower's words. He was almost certainly only concerned that Harriot did not establish his priority and thus a lasting fame (for himself and for his country). There is nothing here to suggest that Lower had already seen in Harriot's work intimations of a new natural philosophy. Furthermore, there is nothing in following correspondence to suggest that he saw this subsequently.

This is instructive. After all, Lower had actually been present at Syon House in December 1610, observing side by side with Harriot, when Harriot discovered sunspots. When reporting this in his biography of Harriot, John Shirley could not refrain from imaginatively reconstructing their excitement:

> To see actual imperfections in a celestial body ostensibly composed of the perfect quintessence of extra-terrestrial bodies was an almost unbelievable experience … [T]he Syon Philosophers shared the excitement of a discovery which, of itself, spelled the doom of the doctrines of the perfection of the quintessence, or circular motion, cycles, and epicycles; and by showing the imperfections of a heavenly body showed the way for acceptance of more material explanations of the operation of the universe.[40]

It is easy to see why Professor Shirley wrote this way. To those of us who have ever undertaken a formal course in the history of science, the really astonishing

[39] Lower to Harriot, 6 February 1610; quoted from Shirley, *Thomas Harriot. A biography*, p. 400.

[40] Shirley, *Thomas Harriot. A biography*, pp. 403–4.

thing about Harriot's discovery of sunspots is that it did not elicit the kind of response that Shirley describes; if what we were told in class is true, then surely Harriot had to be excited at seeing the doom of Aristotelian natural philosophy. Once again we are frustrated to find Harriot merely recording his observations of sunspots without making any significant comment about their significance. What are we to make of this? Is it merely the result of Harriot's 'internal Osiander', telling him that, as an astronomer, he has no ability as a natural philosopher and therefore no right to speculate like one?

Clearly, Shirley was wrong to say that the discovery of sunspots 'of itself' showed the way to a new theory of the cosmos. The discovery evidently required a Galileo, a mathematician *and* philosopher, to show contemporaries what it implied. But Harriot's failure to make of sunspots what Galileo did still seems genuinely surprising. Once again though, perhaps the fault is ours, not Harriot's.

It is possible, after all, to understand why Harriot might not have been as excited as the standard historiographies lead us to suppose he should have been. We simply have to remind ourselves that it was way back in 1572, shortly after Harriot's birth, that Tycho Brahe had famously seen a new star burning so brightly in the sky that it could be seen even in daylight.[41] Having established, by its lack of parallax, that this was indeed a new star and not simply an atmospheric effect, Tycho went on to scrutinize comets in the same way and established that they too gave the lie to the Aristotelian belief that the heavens were unchanging.[42] Even Jesuit astronomers at the Collegio Romano, including Christopher Clavius, conceded that there must be similarities between superlunary and sublunary bodies on the grounds that they do undergo change.[43] Harriot the astronomer therefore grew up in a world where the heavens were not perfect and unchanging, and he must have learned simply to dismiss Aristotelian doctrine on this matter as an irrelevance. We also know that he was one of the very first true Copernicans who accepted the physical truth of the motion of the earth. We can add to this the fact that Harriot probably knew of William Gilbert's map of the moon, which Gilbert included in his unpublished *De mundo*.[44] It is not clear whether Gilbert used a telescope to make his moon map. But when Harriot subsequently came to make

41 T. Brahe, *De nova et nullius aevi memoria prius visa stella* [*On the new and never previously seen star*] (Copenhagen, 1573).

42 T. Brahe, *De mundi aetherei recentioribus phaenomenis* [*Concerning the new phenomena in the ethereal world*] (Uraniburg, 1588).

43 Wallace, 'Traditional natural philosophy', pp. 216–17; and R.R. Volker, '"Whether the stars are innumerable for us?": astronomy and biblical exegesis in the Society of Jesus around 1600', in K. Killeen and P.J. Forshaw (eds), *The word and the world. Biblical exegesis and early modern science* (Basingstoke and New York, 2007), pp. 157–73.

44 W. Gilbert, *De mundo nostro sublunari philosophia nova* [*A new philosophy about our sublunary world*] (Amsterdam, 1651), Chapter XIV, p. 173. Although unpublished at his death in 1603, the *De mundo* may have been written as early as the 1580s; see Sister S. Kelly OSB, *The 'De mundo' of William Gilbert* (Amsterdam, 1965).

his own map with the help of a telescope, he would not have been surprised to see the moon looking like an earthly body, rather than as a glowing disc, or sphere, of unchanging quintessence (indeed, he surely would have been surprised if he had seen anything but an earthly-looking moon).[45] In view of all this, it seems reasonable to suppose that when Harriot saw sunspots, he saw them merely as further evidence for the already well-established changeability of the heavens. Presumably, it simply never occurred to him that anything more needed to be said. After all, even Galileo did not publish on the sunspots he had observed until after the Jesuit astronomer Christoph Scheiner had tried to save the unchanging perfection of the sun by claiming the spots were really satellites orbiting the sun.[46] In short, Harriot responded to sunspots not as we might have expected a natural philosopher to do, but as an astronomer and mathematician.

It seems to me, furthermore, that we must take the same line with all the many different aspects of Harriot's work. He was always thinking as a mathematician: seeking to make measurements, to establish certainties and proofs, and to find pragmatic ends. He was much less concerned, if at all, with providing the kind of explanations in terms of causal narratives that were demanded by natural philosophers. However it may look to us with hindsight distorted by our preoccupations, there was nothing in any of his studies that inevitably had to lead him into making a major revision of traditional natural philosophy. And that being so, we cannot take him to task for failing to turn himself from a brilliantly gifted mathematical practitioner into an innovatory natural philosopher.

Just because we know that Descartes used his mathematical analysis of the rainbow as an example of the success of his philosophy in his *Dioptrics*, which he appended to his *Discourse on method* of 1637, it would be wrong to suppose that Harriot's analysis of the rainbow should have led him to write an English *Le monde*. The link between Descartes's geometrical optics and his system of mechanical philosophy was not direct, in the sense that one did not lead straight to the other. It was only once Descartes had his insight into how a mechanical system of philosophy might work that he was able to provide a new causal explanation of light within the terms of that system. He was then required to show that his new

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45 J.W. Shirley, 'Thomas Harriot's lunar observations', in E. Hilfstein, P. Czartoryski and F.D. Grande (eds), *Science and history. Studies in honor of Edward Rosen* (Wrocław, 1978), pp. 283–308; A. Alexander, 'Lunar maps and coastal outlines: Thomas Harriot's mapping of the Moon', *Studies in history and philosophy of science*, 29 (1998), 345–68; and S. Pumfrey, 'Harriot's maps of the Moon: new interpretations', *Notes and records of the Royal Society*, 63 (2009), 163–8. For a general survey, see E.A. Whitaker, 'Selenography in the seventeenth century', in R. Taton and C. Wilson (eds), *Planetary astronomy from the Renaissance to the rise of astrophysics. Part A. Tycho Brahe to Newton* (Cambridge, 1989), pp. 119–43.

46 Shea, 'Scheiner, and the interpretation of sunspots'; Shea, *Galileo's intellectual revolution*, pp. 49–74; and Moss, *Novelties in the heavens*, pp. 97–125.

account of the nature of light was compatible with geometrical optics, and in that regard he was able to use his analysis of the rainbow to brilliant effect.[47]

Similarly, a study of colliding particles and their reactions does not have to lead to a fully fledged system of natural philosophy. Again, we know that it did in the case of Descartes, but we cannot infer from this that any such study must also lead to a mechanical philosophy. It did not do so, after all, in the hands of Galileo, who originally wanted to include a study of impacts and forces of percussion in his *Discorsi ... intorno a due nuove scienze*, although it is possible that it might have done had he been able to complete it.[48] It did not do so either in the case of Isaac Beeckman, even though Beeckman was specifically trying to develop a new system of physico-mathematical philosophy and even though he came close.[49] If mathematically minded natural philosophers, like Galileo and Beeckman, could not manage it, there were plenty of less ambitious mathematical practitioners who were content to stay within the confines of mechanics. Harriot seems to have been one of these. As Jon Pepper has commented on Harriot's *De reflexione corporum rotundorum*:

> This is in keeping with Harriot's other contributions to science, in which, in my view, the general position which comes through is that of a continual searching for the mathematicizing of the physical world in whatever aspect, with little speculation as such being expressed.[50]

It seems to me that the speculation is crucially important, particularly if we mean speculation about explanations and causes. The only evidence we have that Harriot ever did speculate about causes derives almost entirely from his atomism. Even here, however, we have to rely largely upon a second-hand source,

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[47] For a full account of Descartes's life and work, see S. Gaukroger, *Descartes. An intellectual biography* (Oxford, 1995). See also J-R. Armogathe, 'The rainbow: a privileged epistemological model', in Gaukroger, Schuster and Sutton (eds), *Descartes' natural philosophy*, pp. 249–57.

[48] G. Galilei, *Discorsi e dimostrazioni matematiche intorno à due nuoue scienze, attenenti alla mecanica & i mouimenti locali* (Leiden, 1638).

[49] Cornelis de Waard (ed.), *Journal tenu par Isaac Beeckman de 1604 à 1634*, 4 vols (The Hague, 1939–53); and K. van Berkel, *Isaac Beeckman (1588–1637) en de mechanisering ven het wereldbeeld* (Amsterdam, 1983).

[50] J.V. Pepper, 'Harriot's manuscript on the theory of impacts', *Annals of science*, 33 (1976), 131–51 (133). See also the works by M. Schemmel cited in note 1 above; and S. Clucas, '"No small force": natural philosophy and mathematics in Thomas Gresham's London', in F. Ames-Lewis (ed.), *Sir Thomas Gresham and Gresham College. Studies in the intellectual history of London in the sixteenth and seventeenth centuries* (Aldershot, 1999). For a more general treatment of the topic, see F. Steinle, 'From principles to regularities: tracing "laws of nature" in early modern France and England', in L. Daston and M. Stolleis (eds), *Natural law and laws of nature in early modern Europe: jurisprudence, theology, moral and natural philosophy* (Aldershot, 2008).

Nathaniel Torporley's critique of Harriot's atomism.[51] According to Torporley, Harriot used atomistic precepts to explain three major phenomena: condensation and rarefaction; the refraction of light; and specific gravities. There is no reason to doubt Torporley's account; his claim about the refraction of light is even confirmed by Harriot himself, though only briefly, in a letter to Kepler.[52] So, on the face of it, this seems to provide us with clear evidence that Harriot was willing to play the natural philosopher sometimes.

Yet we are still left in the dark about the extent of Harriot's willingness to play this role. Torporley's *Synopsis of the controversie of atoms* corresponds to no known document written by Harriot himself. It is perfectly possible, therefore, that it represents Torporley's written account of his response to Harriot in what was nothing more than a debate between the two friends in front of other friends. The controversy mentioned in Torporley's title is certainly compatible with a debate carried out on a single occasion.[53] What is more, I find it hard to imagine what Harriot could have said in response to what Torporley calls his 'squadrons' in the battle against Harriot. We need not pursue these arguments here. Suffice it to say that they raised long-standing objections to atomism which would not be overcome until Newton introduced attractive and repulsive forces operating between the atoms.[54] If 'the controversie of atoms' was a specific debate held between Harriot and Torporley, it seems likely that Torporley would have won. If, on the other hand, 'the controversie' represented a long-standing difference of opinion between the two men, it is still hard to believe that Harriot could have had much confidence in his own position in the face of Torporley's squadrons.

Furthermore, if we add to this the evidence provided by Harriot's papers discussing 'infinites' (in which we must include what would later be called infinitesimals), the papers entitled *De infinitis*, it seems hard to imagine that he

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51 N. Torporley, 'A synopsis of the controversie of atoms', British Library, Birch MS. 4458, f. 6 (copied ff. 7–8). This is reprinted in Jacquot, 'Thomas Harriot's reputation', 183–6. Harriot's own discussion of atomistic ideas is chiefly confined to the pages entitled *De infinitis*, and these are predominantly concerned with mathematical rather than philosophical issues. Copies of Harriot's *De infinitis* can be found in the British Library Add. MS 6782b, ff. 362r–74v, BL Add. MS 6785b, ff. 436–7 and BL Harley MS 6002, ff. 2–10. For a fuller discussion of these papers, see Henry, 'Harriot and atomism'; and S. Clucas, '"All the mistery of infinites": Mathematics and the atomism of Thomas Harriot', in S. Rommevaux (ed.), *Mathématiques et connaissance du monde réel avant Galilée* (Montreuil, 2010), pp. 113–54.

52 Harriot to Kepler, 13 July 1608, reprinted as letter no. 497 in J. Kepler, *Gesammelte Werke*, ed. M. Caspar *et al.*, 21 vols (Munich, 1938–2002), vol. 16, pp. 172–3.

53 Henry, 'Harriot and atomism', pp. 286–7.

54 Henry, 'Harriot and atomism'. On Newton's version of atomism, see, for example, R.S. Westfall, 'Newton and the Hermetic tradition', in A.G. Debus (ed.), *Science, medicine and society in the Renaissance*, 2 vols (New York, 1972), vol. 2, 183–98; and R.S. Westfall, 'Newton and alchemy', in B. Vickers (ed.), *Occult and scientific mentalities in the Renaissance* (Cambridge, 1984), pp. 315–35.

could have developed confidence in natural philosophizing by drawing upon atomism. Again, we cannot review all the arguments here. It is sufficient to say that Harriot was led by his mathematical approach to assume that atoms are indivisible by virtue of being indivisibly small, that is, geometrical points. As Aristotle had long since pointed out, however, this kind of mathematical atom cannot be used to compose physical entities. If physical bodies are to be composed of atoms, the atoms must have dimensions and so their indivisibility must be defined in ways that are incompatible with geometrical demands for infinite divisibility.[55] Thus, the evidence suggests that Harriot might have tried his hand as a natural philosopher, using atomism as his explanatory system; yet evidence of internal difficulties and the lack of any signs of development of a fully-worked out system of atomic philosophy also strongly suggest that he found such speculation a frustrating experience, one from which he was perhaps glad to retreat while committing himself instead to the certainties of mathematics.

It might be said that my argument against Harriot developing a system of natural philosophy only works because I artificially keep separate his optics, his theory of impacts and his atomism. If considered together, however, these three strands of Harriot's thought may well indicate a thinker engaged in developing an innovatory system of natural philosophy. Such an argument seems to amount to claiming that Harriot was the English Descartes. The argument would seem to run like this: we know that geometrical optics, the mechanics of colliding bodies, and a matter theory that was closely modelled on atomism all contributed to Descartes's mechanical philosophy, and since we also know that Harriot was involved in these same three projects, he was clearly also involved (before Descartes!) in trying to develop a new system of mechanical philosophy.[56] The hidden premise in such an argument is that the mechanical philosophy follows implicitly from the mere combination of geometrical optics, the mechanics of colliding bodies, and corpuscular matter theory. But this is simply to denigrate Descartes's achievement. This is not the place to try to reconstruct how Descartes arrived at his system of mechanical philosophy, but it certainly did not simply become apparent to him as a result of his pursuing these separate strands of thought in parallel. The mechanical philosophy was forged out of these strands, but in a way that revealed that a unique intellect of great genius was at work.[57] To cite just one example, Descartes's corpuscular matter theory enabled him to adopt aspects of

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[55] On Galileo's use of atomistic arguments, see A. Mark Smith, 'Galileo's theory of indivisibles: revolution or compromise?', *Journal of the history of ideas*, 37 (1976), 571–88; and Henry, 'Harriot and atomism'. Aristotle denies that a collection of indivisibles can make up a magnitude in *Physics*, Book 6, Chapter 1.

[56] On Descartes, see Gaukroger, *Descartes. An intellectual biography*.

[57] Literature on Descartes is vast, but as good a starting point as any is Gaukroger, *Descartes. An intellectual biography*. For a specific example of the innovatory nature of Descartes's work, see J. Henry, 'Metaphysics and the origins of modern science: Descartes and the importance of laws of nature', *Early science and medicine*, 9 (2004), 73–114.

atomism that were highly useful in physical explanation. However, by denying that the invisibly small particles of matter as he conceived them were indivisible, he avoided the insurmountable pitfalls that Torporley's squadrons raised against Harriot's atomism. Harriot was very definitely not the English Descartes.[58]

Therefore, I see Harriot as one of the contributors to the mathematization of the world-picture and to the introduction of experimental techniques, and of mathematical instruments, which went hand-in-hand with that mathematizing process; all of which has been brilliantly described by Jim Bennett and others in recent years.[59] The rise of the mathematical practitioner, intellectually and socially, is undeniably of crucial importance for understanding the scientific revolution, and in this I entirely agree with Bennett. Nevertheless, it should not be forgotten that, as Bennett himself points out, crucial stages in the development of the mechanical philosophy were 'represented by the powerful formulations of individual thinkers'.[60] Those individual thinkers needed to be natural philosophers as well as mathematicians. Where Bennett, quite justifiably, wants to emphasize the role of what he calls the mechanics' philosophy, I want to suggest that to progress from the mechanics' philosophy to the mechanical philosophy, the mechanics needed middle-men, men who were not just mathematicians but who were, like Galileo and like Descartes, mathematicians *and* philosophers. These were the men who recognized the importance of causal explanation as this was emphasized in the Aristotelian tradition and who sought to add physical explanations to otherwise purely mathematical accounts.[61]

It seems to me that only a small handful of thinkers succeeded in combining these two approaches: chief among them were Galileo, Kepler and Descartes, but there were others. We must exclude Harriot from this list, however, on the grounds that his work did not provide the putative causal explanations required

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[58] On Descartes's theory of matter, see W.B. Ashworth, 'Christianity and the mechanistic universe', in D.C. Lindberg and R.L. Numbers (eds), *When science and Christianity meet* (Chicago, 2003), pp. 61–84, esp. pp. 62–5; and P. Dear, *Revolutionizing the sciences. European knowledge and its ambitions, 1500–1700* (Basingstoke, 2001), pp. 80–100.

[59] The 'mathematization of the world-picture' was claimed as characteristic of the scientific revolution by E.J. Dijksterhuis, A. Koyré and E.A. Burtt, and was an insight that proved highly influential. See H. Floris Cohen, *The scientific revolution. A historiographical inquiry* (Chicago, 1994), pp. 59–97; J. Bennett, 'The mechanics' philosophy and the mechanical philosophy', *History of science*, 24 (1986), 1–28; J. Bennett, 'The challenge of practical mathematics', in S. Pumfrey, P. Rossi and M. Slawinski (eds), *Science, culture and popular belief in Renaissance Europe* (Manchester, 1991), pp. 176–90; and Bennett, 'The mechanical arts'.

[60] Bennett, 'The mechanics' philosophy', 24.

[61] The classic account of the 'mathematization of the world-picture' by A. Koyré and others (see note 59, above) sees mathematics being introduced into a previously *qualitative* physics. But the latest historiography presents a story in which causal physical explanations are introduced into mathematical analyses of physical phenomena.

for it to count as natural philosophy. There is no need to be ashamed on Harriot's behalf – he was working, after all, at a time when the separation between natural philosophy and the mixed mathematical sciences was still wide. Even in 1638, when Louis Elzevir published Galileo's *Discourses*, he was using a title chosen by himself. We do not know what title Galileo gave to his last great work, but we do know that he regretted Elzevir's choice of 'a low and common title for the noble and dignified one carried upon the title-page'. In publishing it under the title *Discourses and mathematical demonstrations concerning two new sciences pertaining to mechanics and local motions*, it would seem that Elzevir was playing a similar role to Osiander when he added his apologetic preface to Copernicus's *De revolutionibus*, warning Galileo's readers that this was a book on mathematics. It seems reasonable to assume that Galileo's more 'noble and dignified' title gave the impression that it was a work of natural philosophy. Clearly, Elzevir did not see it that way and felt that his readers would not.[62]

It is also worth remembering that even as late as 1687 the title of Isaac Newton's great book struck contemporaries as puzzling and unfamiliar. To speak of the *Mathematical principles of natural philosophy* was to bring together two intellectual approaches that still seemed very different and widely separated from one another, as far as most educated readers were concerned. What is more, even Leibniz (the German Newton?) objected to what he saw as a lacuna in Newton's natural philosophy, when Newton failed to provide a properly *causal* account of the operation of gravity. As is well known, Newton did not respond by providing one, but by invoking the certainty of the mathematical approach: 'it is enough that gravity really exists and acts according to the laws that we have set forth'.[63] On this occasion, therefore, Newton exploited the lingering division between mathematics and natural philosophy to defend his nescient position on the nature of gravity. This is the kind of response, *mutatis mutandis*, that Copernicus might have given in 1543 if someone had pointed out that he did not explain how the earth moves.[64]

Given that the separation between mathematics and natural philosophy was still so wide, it is hardly surprising that a consummate mathematician like Harriot (surely the superior of Galileo in this regard) should decide to stick to his practice, as a cobbler sticks to his last. After all, he could not have known that the future

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62 See A. Favaro, 'Introduction', in G. Galilei, *Dialogues concerning two new sciences*, trans. H. Crew and A. de Salvio (New York, 1914), p. xii. For further discussion of negative attitudes towards mathematics, see Henry, 'The origins of the experimental method'; and J. Henry, '"Mathematics made no contribution to the public weal": why Jean Fernel became a physician', *Centaurus*, 53 (2011), 193–220.

63 I. Newton, *The Principia. Mathematical principles of natural philosophy*, trans. I. Bernard Cohen and A. Whitman (Berkeley, CA, 1999), p. 943.

64 Indeed, Osiander's defence of Copernicus, in his interpolated prefatory note to *De revolutionibus*, was to point out that he had not offered any explanation of the movement of the Earth and therefore had not transgressed the boundary between mathematics and physics. See above, and Westman, 'The astronomers' role'.

lay with an as yet undreamed of mechanical philosophy, which would be the triumphant result of a combination of the mathematical approach with a natural philosophy that still offered a causal narrative about the way the world worked. It seems far more probable that Harriot would still have seen mathematics and natural philosophy as separate and distinct enterprises. This being so, it is highly likely that he would have believed that the future belonged to mathematical practitioners and that natural philosophy could only lead to dead ends.[65]

Accordingly, I do not think that we have to suppose that Harriot carried around with him an 'internal Osiander', a voice of conscience telling him that as a mathematician he was unsuited for philosophizing.[66] I believe that even the little that we do know about Harriot's atomistic speculations is enough to indicate that he was willing to try his hand at natural philosophy if he thought it would help. The fact that he did not pursue atomism but evidently turned back instead to the certainties of the mathematical approach is not a sign of failure or of diffidence; rather, it is the sign of an uncompromising perfectionism. Harriot compares very favourably here with Galileo and Descartes, both of whom seem to have been astonishingly blind to the obvious failings of their philosophical speculations.

Although Galileo's theory of the tides can be said to show a powerful intuition about the role of oscillatory systems in tidal phenomena, that intuition went far beyond the bounds of his astronomy, his physics, his experimental method and his mathematics.[67] Consequently, the tidal theory, upon which he pinned so much, stood out for his contemporaries as an embarrassing absurdity in his physics. Moreover, the fact that his attempt to explain the perpetual motion of the earth and the other planets depended upon the assumption that the planets move in perfect circles with uniform circular motions seems wilfully dismissive of the demands of astronomy. The whole history of astronomy since the Ancient Greeks had been an attempt to reconcile the belief in uniform circular motions with the all too obvious observations which unavoidably suggest that the planets do not move uniformly or homocentrically. Galileo's argument for the motion of the earth in terms of a perpetual uniform motion on a flat frictionless plane that, unlike a sloping plane,

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[65] Harriot can be likened here to Francis Bacon. Bacon left his readers in little doubt that natural philosophy had so far led only to dead ends, but, not being a mathematician, he called for a reform of natural philosophy. The separation of mathematics from natural philosophy at this time is again underscored by the fact that it never occurred to Bacon to use mathematics in his reform of natural knowledge. See Henry, 'The origins of the experimental method'.

[66] As suggested, remember, by Westman, 'The astronomers' role', 108–9.

[67] See P. Palmieri, 'Re-examining Galileo's theory of tides', *Archive for history of exact sciences*, 53 (1998), 223–375. Palmieri succeeds in showing that Galileo had a powerful intuition about the oscillatory nature of the tides, but the fact remains that Galileo saw these oscillations as the result solely of the motions of the Earth, whereas, as Laplace showed (see Palmieri, p. 356), following Newton, they are set up by the gravitational attractions of the moon and the sun.

affords no reason for the earth to accelerate or decelerate is ingenious to be sure. But once the reader realizes that the 'flat' plane which neither approaches nor recedes from the sun is in fact a sphere around the sun, and that the earth's unceasing motion is therefore proved in terms of its uniform circular motion around the sun, incredulity sets in. It is impossible to read the relevant parts of the *Dialogue on the two chief world systems* without wondering whether Galileo is really serious in developing an argument that does not simply fail to fulfil the Platonic injunction to 'save the phenomena' revealed by astronomy, but egregiously ignores this![68] Similarly, the Cartesian system, for all its breathtaking ingenuity, was shot through with insurmountable difficulties: for example, its insistence that there could be no new motions in the world, but only the transfer of motion from one part of the system of the world to another. And the concept of vortices, as Newton later spelled out, was 'beset with many difficulties'.[69]

We now know, of course, that the failures of Galileo and Descartes were magnificent failures, providing important stages on the way to our modern understanding of the physical world. But their immediate contemporaries could not have known this, much less a thinker like Harriot, who did not live to see the direction that Galileo and Descartes took in their major publications. If Harriot had tried to be a pioneer in combining mathematical approaches with a causal natural philosophy, it is hard to believe that he could have done any better than Descartes. It is likely that his own putative system of natural philosophy, whatever it might have been, would also have proved inadequate (as, for example, that of his fellow 'magus', Walter Warner, did).[70] It seems clear, however, that Harriot never did try to develop a new philosophy; he was perhaps too clever, or too self-critical, to indulge in self-deceiving philosophizing. Throughout his career, he concentrated instead on 'exacte trialls' and other more restricted, but more certain, mathematical approaches, perhaps hoping that new understanding would emerge from the certainties of mathematics.

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[68] Galileo 'demonstrates' that the planets, once set in perfectly circular motions about the sun, would continue to move this way indefinitely without either speeding up or slowing down, in the 'Second Day' of the *Dialogue concerning the two chief world systems*.

[69] Newton, *The Principia*, 'General scholium', p. 939. For an excellent general discussion, see Alan Gabbey, 'The mechanical philosophy and its problems: mechanical explanations, impenetrability, and perpetual motion', in J.C. Pitt (ed.), *Change and progress in modern science* (Dordrecht, 1985), pp. 9–84.

[70] Much work remains to be done on Walter Warner. But for now, consider Jacquot, 'Harriot, Hill, Warner', pp. 117–25; J. Henry, 'Occult qualities and the experimental philosophy: active principles in pre-Newtonian matter theory', *History of science*, 24 (1986), 335–81 (340–42); J. Prins, *Walter Warner (ca. 1557–1643) and his notes on animal organisms* (Utrecht, 1992); S. Clucas, 'The atomism of the Cavendish Circle: a reappraisal', *The seventeenth century*, 9 (1994), 247–73; and S. Clucas, 'Corpuscular matter theory in the Northumberland Circle', in C. Lüthy, J. Murdoch and W. Newman (eds), *Late medieval and early modern corpuscular matter theory* (Leiden, 2001), pp. 181–207.

If he did hold out such hopes for mathematics rather than for philosophical speculation, he could hardly be said to be deluded. Developments in mathematics during the first two decades of the seventeenth century were arguably more exciting, and potentially more fruitful, than developments in natural philosophy. Furthermore, it was at just this time, let us not forget, that Harriot's great contemporary Francis Bacon was also discouraging speculation in philosophy and advocating instead a careful gathering of accurate and reliable information about natural phenomena. Harriot's eschewing of philosophical speculation, so regretted by his modern commentators, was in his day by no means a methodologically unjustifiable position.[71]

In conclusion, I want to say that, in spite of the epitaph on Harriot's monument, where he was described as excelling in mathematics, natural philosophy and theology, he was no more a natural philosopher than he was a theologian, and for that reason he should not be seen as the English Galileo.[72] Furthermore, I believe that he concentrated almost exclusively on mathematics as a matter of personal choice and preference. Although it might be said that in the early part of his career, when he was retained by Walter Ralegh, he had no choice but to pursue the entirely pragmatic ends of his patron, this was not the case during the years for which he was the pensioner of Henry Percy. As Stephen Pumfrey has pointed out, 'Northumberland was unique in patronizing natural philosophy as well as utility', and we know that Walter Warner must have spent much of his time developing the new system of natural philosophy which can be seen in his manuscript remains.[73] Harriot could have pursued natural philosophy if he had wanted to. But, judging from his own manuscripts, he never did.[74] It is impossible to know whether he rejected the natural philosophical approach out of diffidence, brought on by his own 'internal Osiander', telling him that as a mathematician he was unsuited for

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71 The sixteenth century is generally recognized as a period of great achievement in the history of mathematics, but this period is often overlooked in histories of philosophy. The standard account of developments in natural philosophy at this time focuses on what are usually seen as fanciful attempts at system-building by the Italian Renaissance 'nature philosophers' (Telesio, Patrizi, Bruno and Campanella), and the false lights of Paracelsianism and Rosicrucianism: see Brian P. Copenhaver and C.B. Schmitt, *Renaissance philosophy* (Oxford, 1992). Literature on Bacon's would-be reforms in natural philosophy is vast, but see, for example, S. Gaukroger, *Francis Bacon and the transformation of early-modern philosophy* (Cambridge, 2001).

72 For the full transcription, and a translation from the Latin, see Shirley, *Thomas Harriot. A biography*, p. 474.

73 Pumfrey, 'Was Harriot the English Galileo?', p. 18. On Warner, see the articles cited in note 70 above and Shirley, *Thomas Harriot. A biography*.

74 With the possible exception, as was said earlier, of a brief and inconsequential flirtation with atomism.

philosophy, or as a result of a perfectionism that he felt he could achieve through mathematics, but not through the much less certain speculations of physics. Either way, Harriot was not the English Galileo.

Chapter 7

Patronizing, Publishing and Perishing: Harriot's Lost Opportunities and His Lost Work 'Arcticon'

Stephen Pumfrey

During the 1580s, Thomas Harriot compiled an advanced work on navigation called 'Arcticon'. Like all of his mathematical work, it remained in manuscript, and it was one of the manuscript works that are now lost. John Roche has stated that, had it been published, it 'would have had an immediate impact on western navigation and established Harriot internationally as a navigation expert'.[1] By contrast, Harriot's close contemporary Edward Wright achieved in the seventeenth century the level of international recognition as a practical mathematician that Harriot's brilliance deserved but did not receive. Wright's fame rested, and rests, on a treatise very similar to the lost 'Arcticon', his *Certaine errors in navigation*, of which the first of several editions was printed in London in 1599. Mark Monmonier considers that '[i]f Thomas Harriot had been as eager to publish, Edward Wright might be no better known today than Abraham Kendall or Henry Bond'.[2] In this chapter I will analyse and compare the fortunes of Harriot and Wright and their navigational treatises. I do so with specific attention to two factors: the system of early modern patronage within which both men worked and the vagaries of publishing at the time, where manuscript circulation still vied with printed editions. My revisionist conclusions are that Harriot's lack of publications was normal, whilst Wright's monumental publication was an accident of history.

'*Doctissimus ille Harriotus de Syon ad Flumen Thamesin Patria & educatione Oxoniensis*': this was Harriot's epitaph.[3] His career path, from sober *magister* of the University of Oxford to innovative client of a London grandee (Syon House belonged to his patron the Earl of Northumberland), is familiar to students of the patronage of science in early modern England. Elizabeth I's dynamic metropolis with its courtly patrons offered exciting prospects for ambitious Oxbridge-trained

1 J.J. Roche, 'Thomas Harriot's astronomy' (DPhil thesis, University of Oxford, 1977), p. 147, cited by J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), p. 95.

2 M.S. Monmonier, *Rhumb lines and map wars. A social history of the Mercator projection* (Chicago, 2004), p. 74.

3 Cited and translated by Shirley, *Thomas Harriot*, p. 474: 'that most learned man of Syon on the River Thames, by birth and by education an Oxonian'.

intellectuals.[4] The same path to success was travelled by many of the influential friends and acquaintances whom the low-born Harriot met in Oxford, notably his fellow mathematicians Robert Hues, Walter Warner and Nathaniel Torporley, the geographer Richard Hakluyt, the poet George Chapman, as well as Harriot's first patron Sir Walter Ralegh and Ralegh's loyal lieutenant Laurence Keymis. A comparable path, from the University of Cambridge to London, was taken by Edward Wright.

We became acutely aware of Harriot's failure to achieve the recognition accorded to Wright as this volume was being compiled during 2009 and 2010, when the 400th anniversary of telescopic astronomy was being celebrated. Whilst the twenty-first-century world was familiar with the achievements of Galileo Galilei, few (even in Britain) realized that it was Harriot who was the first to turn a telescope to the heavens. The difference is explained, of course, by the fact that Galileo printed his observations in a masterful work of self-promotion, his *Sidereus nuncius* of 1610, whilst Harriot confined his to manuscripts that were known to very few until 1965.[5]

Harriot did groundbreaking work in several areas, but he did not turn it into printed books. Although his celebrated *Briefe and true report of the new found land of Virginia* was printed in London in 1588 when he was only 28, nothing else went to press until 1631, when Walter Warner published a selection of Harriot's mathematical papers as the posthumous tract *Artis analyticae praxis*.[6] It is only from Harriot's manuscripts that we have come to appreciate the strength of the often-made claim that he was 'the English Galileo'.[7] He became an unsurpassed expert in advanced navigation and cartography; independently of François Viète, he arrived at the sine law of refraction in optics, and he turned the Frenchman's algebra into a fully symbolic form. He used his telescope to observe the lunar

[4] A more positive assessment of Oxbridge is articulated in M. Feingold, *The mathematician's apprenticeship. Science, universities and society in England, 1560–1640* (Cambridge, 1984).

[5] For a comparative review of Harriot's and Galileo's work, see S. Pumfrey, 'Harriot's maps of the moon: new interpretations', *Notes and records of the Royal Society*, 63 (2009), 1–6.

[6] Thomas Harriot, *Artis analyticae praxis, ad aequationes algebraïcas nouâ, expeditâ, & generali methodo, resoluendas: tractatus e posthumis Thomae Harrioti philosophi ac mathematici celeberrimi schediasmatis summâ fide & diligentiâ descriptus: et illustrissimo Domino Dom. Henrico Percio, Northumbriae comiti, qui haec primò, sub patronatus & munificentiae suae auspicijs ad proprios vsus elucubrata, in communem mathematicorum vtilitatem, denuò reuisenda, describenda, & publicanda mandauit, meritissimi honoris ergò nuncupatus* (London, 1631). Note how the title emphasizes Harriot's patronage by Henry Percy, the ninth Earl of Northumberland. Warner had also been his client and after Henry's death was maintained by his son and heir Algernon Percy, the tenth Earl.

[7] For a recent re-statement of the claim, see M. Schemmel, 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', *Physics in perspective*, 8 (2006), 360–80.

surface, to discover sunspots and to determine the period of Jupiter's satellite Io. He was also an early Copernican and a developer of matter theory who combined experimental research in alchemy with a philosophy of atoms and voids that some found impious. However, as John Roche has shrewdly observed, he left too many projects immature and flawed, and ignored the fact that 'the commitment to publication often brings a scholar's work to maturity'.[8]

Why did Harriot not publish and procure a reputation? This question has been asked for 400 years by those sympathetic to him. As is well known, his philosophical protégé and patron Sir William Lower asked it in February 1610 when he had read Johannes Kepler's *Astronomia nova* as Harriot had recommended:

> Do you not here startle, to see every day some of your inventions taken from you; for I remember long since you told me as much, that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it in print, a little before Vieta prevented you of the gharland of the greate Invention of Algebra, al these were your deues and manie others that I could mention ; and yet to[o] great reservednesse had robd you of these glories, but although the inventions be greate, the first and last I meane, yet when I survei your storehouse, I see they are the smallest things and such as in comparison of manie others are of smal or no value. Onlie let this remember you, that it is possible by to much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your Countrie and frinds injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise workes, but you know best what you have to doe. Onlie I, because I wish you all good, with this, and sometimes the more longinglie, because in one of your letters you gave me some kind of hope therof.[9]

Previously I have suggested in general terms that considerations of patronage go a long way towards explaining why the Tuscan Galileo acquired fame and influence while the Oxonian Galileo did not.[10] I argued that Harriot's two big patrons, Sir Walter Ralegh and Henry Percy, the ninth Earl of Northumberland, were too politically compromised to advance their client properly. In his recent biography, John Roche observes that the failure to publish 'was in part due to Harriot's reputation for impiety and his close association with Ralegh and Northumberland, but it is difficult to see how publications in navigational science or of maps, mathematics, optics, mechanics, or astronomy could have been other

[8] J.J. Roche, 'Harriot, Oxford, and twentieth-century historiography', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 229–45 (p. 235).

[9] Quoted in Shirley, *Thomas Harriot*, p. 400.

[10] S. Pumfrey, 'Was Thomas Harriot the English Galileo? An answer from patronage studies', *Bulletin of the Society for Renaissance Studies*, 21 (2003), 11–22.

than beneficial to his position'.[11] In this chapter I offer an explanation as to why Harriot did not publish (or, to be precise, did not have printed) even a navigational and cartographical achievement like 'Arcticon'.

It would seem that Harriot compiled his lost treatise of navigation around 1584, when he was training Ralegh's sea captains for their voyages to Virginia. We have some idea of its content because his manuscript notes survive from similar lectures or 'Instructions for Raleigh's voyage to Guyana' in 1595, which make reference to, for example, 'the demonstration which I have uttered in the Arcticon, which here for brevity sake I omit'.[12] In his 2004 analysis of Elizabethan works of navigation, Eric Ash states that 'the manual itself was presented to Raleigh directly, and unfortunately it is now lost'.

Ash praises Harriot's unrivalled 'lucid organisation and presentation of complex information [in] as clear and logical fashion as could ever be hoped for', and compares Wright's *Certaine errors* with it unfavourably.[13] From the six Guyana 'Instructions', we can infer that 'Arcticon' sought to correct some of the same key errors as Wright's book. Harriot included lectures on the errors of the cross-staff and the declination of the sun, which also made up the third and fourth of the four parts of Wright's *Certaine errors*. Harriot called his first error of the cross-staff the 'parallaxis of the staff', while Wright called it the 'eccentricitie or paralax of the eye'. Ash notes that 'Harriot's approach to correcting the use of the cross-staff obviously has much in common with Wright's'. Perhaps the most remarkable overlap concerned cartography, specifically the obscure mathematics which lay behind the new charts of Gerard Mercator. Wright's revelation of it was a major aspect of the first part of *Certaine errors*, 'wherein are set downe the errors of the common Sea chart'.[14] In Monmonier's judgement, Harriot's unpublished solution 'is cleaner and more mathematically elegant insofar as he had progressed from merely adding up secants, as Wright had done, to a logarithmic tangents formula that affords a more exact and direct solution'.[15] Harriot and Wright were clearly working independently to very similar agenda as they advanced the highest technological standards in navigation.

In contrasting the sad history of Harriot and his 'Arcticon' with the success story of Wright and *Certaine errors*, I find my argument that patronage factors were crucial in the careers of Harriot and other Elizabethans confirmed. In regard

11 J.J. Roche, 'Harriot, Thomas (c. 1560–1621)', in *Oxford dictionary of national biography*, 60 vols (Oxford, 2004).

12 Cited in Shirley, *Thomas Harriot*, p. 95.

13 E.H. Ash, *Power, knowledge and expertise in Elizabethan England* (Baltimore, MD, 2004). For Harriot, see pp. 169–76. Quotations from pp. 172 and 173.

14 Ibid., pp. 169–76. For Wright see E[dward]. W[right]., Certaine errors in navigation, arising either of the ordinarie erroneous making or using of the sea chart, compasse, crosse staffe, and tables of declination of the sunne, and fixed starrres detected and corrected (London, 1599), sigs N3v, ¶¶v, ¶¶¶3v.

15 Monmonier, *Rhumb lines and map wars*, p. 72.

to navigation, however, they operated in a more complex way than I had first expected. As we shall see, Wright was no Galileo, seeking publication in order to secure his reputation. It turns out that Harriot was not very different from Wright, John Dee and other clients who offered their entrepreneurial patrons navigational expertise and who were expected to keep secret any competitive advantages. However, when circumstances such as pre-emption or plagiarism made printing an appropriate strategy, Wright had the requisite patronly support and Harriot did not.

In Harriot's time, the support of patrons was crucial if a client's work was to make the transition from private consumption (by a circle of readers of a few manuscript copies or by direct personal instruction) to public recognition via an authoritative and printed edition. As we will see below, the Earl of Cumberland, Wright's patron, played a vital role in helping him to secure his reputation and intellectual property, whilst often Ralegh was unwilling and Northumberland was unable to do the same for Harriot. The contrasts perfectly illustrate the role of patronage in Elizabethan science. They require us to ask different questions, to stop asking why did Harriot *not* publish and instead to ask why Harriot *should* have published, *could* he have published if he had wanted to and why did Wright publish eventually? Harriot therefore offers a case study of the more general question of how scientific works were circulated and printed in his time.

Harriot's life spanned a major change in the culture of dissemination of knowledge, especially scientific knowledge. The transition, part of the more general transition from the Renaissance to the early modern period, involved a shift from a predominantly manuscript to a predominantly print culture. Nevertheless, manuscript circulation remained significant well into the seventeenth century, as Love and Marotti have shown. Circumstances remained where manuscript was preferred to print. It communicated more individually and intimately. Elites eschewed print because fixed, multiple copies were associated literally with common subjects and an ungentlemanly pursuit of fame. The shift was slow, barely begun in Harriot's time, from the semi-private exchanges between a client, his patron and the patron's circle to the commercial exchanges between a practitioner and his book-buying, instrument-buying or medicine-buying public.[16] Likewise far from complete was the shift from what William Eamon has called 'the secrets of nature', offered by a client to his patron's circle alone, to 'science', made publicly available by named discoverers like Galileo to a national or international audience.[17] Historians of science have overlooked the extent to which Harriot, Wright and their contemporaries were content with manuscript circulation, and the obstacles, especially for Harriot, which stood in the way of moving their work from private, patronly and manuscript culture into a public print culture.

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[16] H. Love and A.F. Marotti, 'Manuscript transmission and circulation', in D. Loewenstein and J. Mueller (eds), *Cambridge history of early modern English literature* (Cambridge, 2002), pp. 185–203.

[17] W. Eamon, *Science and the secrets of nature. Books of secrets in medieval and early modern culture* (Princeton, NJ, 1994).

Had Harriot taken his friend Lower's advice and sought to have some of his work printed, convention would have required him to do the following. First, he would have had negotiations with brokers such as the secretaries to Ralegh or Northumberland. They would have explored whether the patron wanted the work published and whether he wanted his name associated with it. If he did, then he would normally become the dedicatee. Secondly, Harriot would have had a scribe prepare a fair manuscript copy of the work to be presented ritually as a gift to the dedicatee, as 'Arcticon' seems to have been to Ralegh. It would have been accompanied with a semi-public letter of dedication to be read by, or read out before, the patron. Other manuscript copies of the work would also have existed (not least copies used by navigators) and readers of them would have helped the patron and his circle to establish the value of the work. These copies could also arrive in the hands of rivals or enemies. Assuming that the patron was advised to accept the dedication, and further assuming that the expense of a printed edition of the work was considered mutually advantageous to both the patron and the client, the manuscript would have been sent to a press, complete with the letter of dedication, a further letter 'to the reader' and other appropriate front matter establishing the credentials and scope of the work.[18]

Letters of dedication were very formulaic. Indeed, in Harriot's time all letter writing, from letters pressing political suits to those asking merely to borrow a book, were governed by rules. Epistolary manuals such as *The English secretorie* expounded these rules, which were followed by Wright and other authors.[19] While researching science and patronage in early modern England, I have read the front matter of nearly 1,000 works dealing with natural knowledge, and I find that they conform to general rules while also tailoring aspects to the specific subject. After a flattering address, clients would often use a classical or biblical quotation to signal the importance of their field. Practical mathematicians found *Wisdom* 11:21 useful, where Solomon declared that 'Thou, O Lord, hath disposed all things in number, weight and measure'.[20] Allusion was then made, if possible, to signs of the patron's past support of the client's work – a stipend received, an instrument donated or a previous dedication accepted. No matter how small these were, the noble would be compared favourably (if absurdly) with Maecenas, the archetypal patron and intimate of Virgil and Horace.

The client would then describe how and why he had come to offer his book as a gift to his patron. Convention required him to explain how he, with his inferior abilities, had nothing fit for a lord. He may have penned a few works, but in his

18 For an overview of the patronage system, see S. Pumfrey and F. Dawbarn, 'Science and patronage in England, 1570–1625: a preliminary study', *History of science*, 42 (2004), 137–88.

19 A. Daye, *The English secretorie* (London, 1586).

20 See, for example, T. Bedwell, *Mesolabivm architectonicvm, that is, a most rare, and singular instrument, for the easie, speedy, and most certaine measuring of plaines and solids by the foote* (London, 1631), sig. A2r.

judgement they were not worthy of wider circulation or printing. The client would often state that friends, or the patron himself, had overridden his judgement and pronounced them to be of value. The ultimate authority and guarantor of their worth was the patron, to whom the actual author would ascribe implausible intellectual accomplishments and judgement. Finally, the author would ask for the patron to defend him against his critics. We will see all these features at work in several dedications to works of navigation, including Wright's *Certaine errors*, which I will consider in detail at the end of this chapter. We can assume that Harriot would have followed these conventions had the 'Arcticon' survived.

It is a particular feature of late sixteenth- and early seventeenth-century England that authors express general concern about critics, who are characterized as malicious, venomous and jealous. Between 1550 and 1610, it was the fashion of English writers to personify these anonymous critics as Zoilus and Momus. Zoilus was a contemporary of Plato and Aristotle who was famous for his very contrarian opinion that Homer's poetry was deeply flawed. Momus is more interesting and relevant to mathematical practitioners like Harriot and Wright. He was a Greek deity, specifically the god of fault-finding, first mentioned in Hesiod's *Theogony* (c. 700 BC), then in one of Aesop's Fables and at more length by Lucian. His story was expanded and reinvented for the Renaissance by the Italian polymath and (appropriately enough) mathematical practitioner Leon Battista Alberti in a satire that circulated in manuscript form around 1450 and was published posthumously in 1520.[21]

In the story of Momus, some deities are represented as client inventors of Jupiter, who offer him their inventions of a house, a cow and a man with which Jupiter can fill his newly created world. Only Momus dared to find fault with the designs. The house, he said, should have had wheels so that it could be mobile, the cow's eyes should have been situated under its horns so that it could see better when charging and the man should have had a window opening onto his heart so that his true motives could be known. Momus gave Jupiter a creation fitting his personality. He filled Jupiter's 'world with bugs, wasps, moths, hornets, cockroaches and other nasty little creatures similar to himself', similar because their biting mouths caused intense irritation. As a result, Momus was expelled to live on earth, from where he encouraged Rumour to spread discord and irreligion among men.[22]

The front matter of books published in England during Harriot's lifetime give the impression of a plague of biting and carping critics in these decades.[23] There were

21 L.B. Alberti, *Leonis Baptistae Alberti Florentini Momus* (Rome, 1520).

22 L.B. Alberti, *Momus*. English and Latin., English translation by S. Knight. Latin text edited by V. Brown and S. Knight (Cambridge, MA, 2003), pp. 15–19.

23 See S. Pumfrey, 'Managing Momus; following the fortunà and frequency of a trope in early English books online', paper presented to the AHRC ICT Methods Network, 22 July 2006; and P. Rayson et al., *Workshop on text mining*, available at: http://ucrel.lancs.ac.uk/events/htm06/PumfreyHTM06.pdf.

several common criticisms with which authors of works of practical mathematics anticipated that their Momuses and Zoiluses would greet the publication. These included that it is not learned enough; or, conversely, that such learning should have been published in the learned tongue of Latin and not in the English vernacular; that the work is derivative or worthless; or, conversely, that the author is seeking to profit personally from the illicit publication of private 'secrets'. Insisting that such criticisms are baseless and malicious, the client then begs the patron to accept the dedication, because the patron's honour will be sufficient to close the mouths of his client's detractors. It will also reinforce the client's protestation that he publishes his labours not for himself but for the commonwealth, a protestation often expressed through Cicero's Platonic maxim 'non nobis solum nati sumus ortusque nostri partem patria vindicat, partem amici'.[24]

A typical conclusion had the author declare that, if this work found favour, the client had other, better and more fitting works in hand to offer the patron. To bring these projects to completion would need further support, which he subtly canvassed. He would close by pledging his devotion to and beseeching God to favour the patron and his family.

This brief overview of letters of dedication allows us to develop four conclusions relevant to scientific and technical books of the period like Harriot's 'Arcticon'. The first is that the specialist works of a client were routinely first produced and circulated for a manuscript culture. Initially they circulated within the semi-private entourage of the client's patron. Copies would be passed on to other interested scholars and readers, and sometimes found their way into the hands of rivals or critics.

Two kinds of book ended up in print. The first comprised works on popular subjects. They were produced somewhat independently of the system of patronage and often crudely printed, with a public print culture in mind. Marotti has argued that it was only towards the end of Harriot's life, as a commercial, *printed* book-buying culture developed, that 'the public' emerged as a kind of collective patron capable of offering financial support.[25] But this public was not a significant audience for the kind of specialist work that Harriot was doing. His works of practical mathematics belonged to the second kind, those which depended upon financial support. Mathematical works were often expensive to produce: drawings of instruments and diagrams of geometrical operations could be especially costly. With a limited audience they needed a patron to fund the cost of printing, and the funds were not always provided.

Harriot's 'Arcticon' was one of a large number of Elizabethan works of science and technology that existed only in manuscript form. It is comparable to 'The Jewell of Artes' by George Waymouth, which he dedicated to James I in 1604 and of which a superb manuscript presentation copy survives in the British Library.

24 Cicero, *De officiis*, 1:22.

25 A.F. Marotti, 'Poetry, patronage and print', in C.C. Brown (ed.), *Patronage, politics, and literary traditions in England, 1558–1658* (Detroit, 1993), pp. 21–46.

Little is known of Waymouth's career, but he seems to have been a minor Harriot. His recent biographer records that he was a studious mathematician and a skilled designer and navigator who took part in voyages of exploration and colonization: he even brought back to England five Abenaki Indians with whom he had traded on the coast of Maine.[26] Waymouth's 'Jewell' treated navigation, maps and ordnance, key subjects for Harriot and Wright too. His unprinted dedication is a model of conventionality, right down to his 'referring the publishing … only to your majesties high prudence and discretion', as to whether such knowledge was best printed or kept private. Unconventionally, it explicitly makes a suit for 'some maintenance and imployment at home: or … abroade'. Although the 'Jewell' was not printed, Waymouth's suit succeeded and he received commissions to lead expeditions to Chesapeake Bay.[27]

If practical and useful knowledge often did not get to the English press, neither did more speculative natural philosophy. An example known to Harriot (and Wright) concerned William Gilbert, author of the printed success story *De magnete*, for which Wright wrote a 'laudatory address'. A short while after Gilbert died in 1603, his half-brother formally presented a posthumous collection of Gilbert's writings, *De mundo*, to Henry, Prince of Wales. The dedication offered it 'either for public use or for preservation in your library'.[28] Informing Kepler of its existence in a letter of July 1608, Harriot noted that he expected it to be published before the end of the year. It was not, and Harriot and Francis Bacon are the only contemporaries we know to have read Gilbert's work in manuscript form. Harriot was very interested in Gilbert's controversial advocacy in *De mundo* of an infinite Copernican universe, an interstellar vacuum and hence a new theory of light, topics on which, of course, his own ideas remained unpublished, as did Gilbert's.[29]

In Harriot's time, then, much written scientific knowledge circulated informally. There were many more works than the few that were printed or that left a manuscript presence. As Deborah Harkness has pointed out, also important were the informal networks of London's artisans and practitioners, who left few written traces, let alone printed ones.[30]

The second conclusion is that we should not dismiss claims by an author of a printed work that it was one of several they have composed, and that if the patron and readers approve, there are more and better books to come. Of course, such assurances of virtually completed masterpieces included examples of the

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26 D.R. Ransome, 'Waymouth, George (*fl.* 1587–1611)', *Oxford dictionary of national biography* (Oxford, 2004).

27 Ibid.; G. Waymouth, 'The Jewell of Artes', BL MS Add. 19989.

28 'Vel in publicos usus exponendam, vel in bibliotheca tua vere regia custodiendam', in W. Gilbert, 'De mundo nostro sublunari', BL Royal MS 12.F .XI., f. 1v.

29 Harriot to Kepler, 13 July, 1608. Quoted in J.W. Shirley (ed.), *Thomas Harriot. Renaissance scientist* (Oxford, 1974), p. 4.

30 D. Harkness, *The jewel house. Elizabethan London and the scientific revolution* (New Haven, 2007).

perennial self-deception and broken promises which afflict intellectuals who rely upon the publication of their work. In any case, given the insecure and transient nature of noble patronage, such assurances were clearly a good tactic as clients sought to maintain or increase favour. Yet, in some cases, the historian of science can confirm both that other works existed and that only a small proportion of them made it into print.

In a very early example, the *Cosmographicall glasse* of 1559, William Cunningham informed his patron Robert Dudley that:

> if it shall please your honore to take this simple worke into your tuition, and be Patrone unto it: I shall be bouldened (God graunting me life) to present you also wyth other of my laboures, the Titles of which foloweth [there are seven, including 'The Astronomical Ring'] … with divers others whose names I omit for sondry causes.

None survived. We are luckier with another of the first authors to have works of practical mathematics printed in English: Leonard Digges. He promised other mathematical work in his *Tectonicon* of 1556. The most important appeared posthumously when his son (and John Dee's protégé) Thomas Digges completed it as *Pantometria*, which he had printed in 1571. Thomas Digges himself promised several works of his own which never appeared and are presumably lost. Significantly, first on the list was 'A Treatise of the Arte of Navigation, bewraying the grosse Erroures by our Maysters, and Marriners practised'.[31]

One could multiple examples, but one worth mentioning is John Dee. Dee did nothing by halves. In his 'Compendious Rehearsall', an open letter read to Elizabeth I as part of his desperate campaign for renewed patronage and later printed, Dee listed 48 works, of which only eight were printed. Some of the 40 'unprinted Bookes and Treatises' are known to have existed, such as a manuscript treatise on sea power composed for Sir Edward Dyer. Some may have been aspirations only: Dee once recorded in his diary: 'The morning as I lay in my bed it cam into my fantasy to write a boke, *De differentiis quibusdam corporum et Spiritum.*' But there is no reason to doubt that Dee really wrote his fourteenth item, 'The Astronomicall, & logisticall rules, and Canons, to calculate the Ephemerides by, and other necessary accounts of heauenly motions: written at the request, and for the vse of that excellent Mechanicien Maister Richard Chauncelor, at his last voyage into Moschouia'.[32]

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[31] W. Cunningham, *The cosmographical glasse, conteinyng the pleasant principles of cosmographie, geographie, hydrographie, or navigation* (London, 1559), sig. Aii verso; L. Digges, *A boke named Tectonicon* (London, 1556), 'L.D. unto the Reader'; T. Digges, *An arithmeticall militare treatise, named Stratioticos* (London, 1579), sig. Aiv ff.

[32] J. Dee, *A letter containing a most briefe discourse apologeticall* (London, 1599); J. Dee, 'The compendious rehearsall of John Dee', in J. Crossley (ed.), *The autobiographical tracts of John Dee* (London, 1851), pp. 26–7; I.R.F. Calder, 'John Dee studied as an English

These examples establish that Harriot would not have been unusual or negligent in leaving a collection of semi-completed and unpublished treatises which have since been lost. What is unusual about Harriot is that, the *Briefe and true report* aside, none of his treatises achieved the wide circulation and recognition that print could confer.

As a third conclusion, we must remind ourselves of the crucial role of the patron in authorizing a book to move fully into the public sphere as a printed edition. Especially for a work like Harriot's 'Arcticon' or Wright's *Certaine errors*, the patron was important in three ways. In the first place, a lot of the client's work that went into such books was made possible by the patron's provision of financial and other resources. The patron therefore had rights over its use and distribution. This was especially true when the work conferred a clear advantage, for example, in military or economic terms. The patrons of navigational experts, as Ralegh was of Harriot, Cumberland of Wright, and Sir Francis Drake of the esteemed navigator Abraham Kendall, were ruthless and competitive privateers. They were in open conflict with foreign fleets and in tacit competition with each other. We will see that Ralegh and Cumberland did not encourage publication of their navigators' secrets. In the second place, the patron often paid for the printing, especially of books with unusual content, such as the phrases in Greek and Hebrew beloved of humanists or the complicated diagrams drawn up by mathematical practitioners, which created expenses well beyond those which could be borne by the client or printer alone. Finally, the authority of a book, especially an innovative one, was established by the letter of dedication which made clear the involvement of a patron, whose supposed honour and discernment provided the Renaissance equivalent of the authority conferred today by a peer-reviewed academic journal. This authority was especially vital if the work attracted controversy, as did Wright's *Certaine errors*.

The crucial authority of the patron goes a long way towards explaining Harriot's lack of publications. Sir Walter Ralegh, never a helpful patron, was out of favour in Elizabeth I's court from the 1590s. Under James I, both Ralegh and Harriot's second patron Henry Percy spent most of their remaining years imprisoned in the Tower of London on suspicion of atheism and treason. In short, neither had any viable honour to lend to Harriot and his work, despite being his only obvious dedicatees.[33]

Of course, it was open to Harriot to find other patrons who could have promoted his work. There is evidence that in the late 1600s he cultivated Henry, Prince of Wales, the Prince's young friend John Harington, and the Earl of Salisbury. All had died by 1613. I do not know whether or not they would have supported publication, but, like John Dee, Harriot had a tainted reputation which made it difficult for a patron to be identified with him. After all, by 1605 he had the status of being a conjuror, an associate of Catholic traitors and so theologically heterodox that

Neoplatonist '(PhD thesis, University of London, 1952), Chapter 10, sect. 3. It is available online at: http://www.johndee.org/calder/html/TOC.html.

[33] Pumfrey, 'Was Thomas Harriot the English Galileo?'.

during Ralegh's trial he was singled out by the Lord Chief Justice as 'an atheist and evil influence'.[34] In patronage culture, of course, reputation and honour ran both ways: the patron lent honour to the client, but the client's reputation also affected the patron's honour. I suspect that Harriot, for all his brilliance, was too tainted.

This brings me to the fourth and final conclusion I want to draw from the nature of dedications. I argue that the Momuses and Zoiluses mentioned in them were not always mere tropes. Paula Findlen has described the culture of patronage in Italy as 'a competitive system of cultural exchange', and Harriot's England seems to have been little different in this regard. The successful client lobbied hard, first to get patronage and then to maintain it and to increase it at the expense of his rivals. As Biagioli has shown for the acerbic Galileo, even long-established[35] and successful players of the patronage game could suffer 'the fall of the favourite' as new talent pressed for its reward.[36] Harriot also knew well from Ralegh's spectacular fall that competition *between patrons* for the ruler's favour could result in the sudden fall of clients. Mathematicians were not immune from 'backbiting' nature of patronage culture.

All four of the above conclusions are nicely exemplified by Robert Tanner in his appallingly alliterative work *The mirror for mathematiques*. Tanner dedicated this book of 1587 to Charles Howard, Lord Effingham, who was Lord High Admiral from 1585 to 1619. Obviously, many writers of nautical works wanted Howard's patronage. Indeed, it played a role in the publication of *Certaine errors*. With a keener eye for new patrons than Harriot, Wright dedicated to Howard his translation of the Dutchman Simon Stevin's longitude scheme *De Havenvinding*, which also appeared in 1599.[37]

Tanner presented himself conventionally as the hesitant writer who had 'doubtfulness, whether it were best to write, and so to showe my good will, or to suspend my pen, and so to hyde the same'. Tanner knew that he might be accused of selfish rather than virtuous motives because, as Momus himself had pointed out, 'Jupiter hath not made a mans body like a Lattise, that the hart might be seen through the holes'. He had made the Lord Admiral his dedicatee because he was 'a personage most meete, and bulwarke most sufficient, against the barking imps of the envious, and blunt hasty boltes of the foolish, who … are not able to penetrate the subtiltie of Mathematicall sciences, and therefore eyther as slothfull neglect them, or as malicious wholy contemne them'. By contrast, Howard possessed honour, learning and wisdom. Tanner add the conventional caveat that 'I lack time

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[34] Shirley, *Thomas Harriot*, pp. 316–17.

[35] R[obert] T[anner], *The mirror for mathematiques. A golden gem for geometricians. A sure safety for saylers* (London, 1587).

[36] P. Findlen, *Possessing nature. Museums, collecting and scientific culture in early modern Italy* (Berkeley, 1994), p. 347; M. Biagioli, *Galileo, courtier. The practice of science in the culture of absolutism* (Chicago, 1993), p. 313.

[37] E. Wright, *The havenfinding art* (London, 1599).

to alter, skyll to frame, and knowledge to reduce [the *Mirror*] to a perfect shape, requiring your honor to esteeme it, as it is, and favourably to hide what so you find amisse'. Finally, Tanner hoped that if Howard received, defended and read 'thys rude work, which I offer to your honour, as an outwarde signe of myne inwarde goodwill, so shall you encourage me after, to present your honor with some worke of more importaunce'.[38]

Since dedicatory epistles were stylized, we must be cautious about reading anything specific into authors' complaints about carping critics like Momus and Zoilus. One could argue that allusions to critics were required by the discursive structure of the dedication: if it was obligatory to invoke the protection of the patron's honour, then that honour needed the existence of dishonourable detractors for it to have a function. However, in Harriot's time the London intellectual community was full of real critics and rivals. Harriot was one of a rapidly growing number of mathematical and other practitioners who laid themselves open to objections of various kinds. A devastating and personal accusation was that the client had stolen his work from another practitioner. In due course we will come to the real and serious charges and counter-charges of plagiarism that Edward Wright was involved in, but first we will review some more general attacks.

Supporters of the traditional learning of Oxford and Cambridge and the London College of Physicians declared many of the new books of practical mathematics and their authors to be insufficiently learned. Their target was not just practical mathematicians: surgeons, apothecaries and other self-styled Paracelsian healers were attacked, men like the alchemists John Hester, Thomas Hill and John Forester, and the surgeons William Clowes, George Gale and George Baker. They used the printed front matter of letters to their patrons and readers to fight back. Thus, in his dedication 'To the worshipfull the Maister Wardens, and generall Assistants of the fraternitie of Chirurgeons in London', Hester contrasted his practical expertise with those 'writers now adaies, who think him the best clark that voucheth the most [ancient] authors and, not pondering their proofs, run hudling on with their *ipse dixit*'.[39]

Another source of complaints by conservative scholars was the suitability of the English vernacular to convey profound subjects like mathematics. In 1559 William Cunningham had confidently expressed the hope that his *Cosmographical glasse* would not 'be rewarded with ingratitude, or ill reporte. And if for the difficultie of the worke, any errour escape: remember I am the firste that in our tongue have written of this argument'. By 1563 he reported that he been attacked by 'malevolent detractors' who presented themselves as learned but were not, yet he urged the surgeon Thomas Gale to ignore them and publish.[40] Thomas

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38 R[obert] T[anner], *A mirror for mathematiques* (London, 1587), sigs A.ii–iv.

39 J. Hester, *An excellent treatise teaching howe to cure the French-Pockes* (London, 1590), sig. ¶.ii.r.

40 Cunningham, *The cosmographical glasse*; and W. Cunningham, 'unto his approved frende Thomas Gale', in T. Gale, *Certaine workes of chirurgerie* (London, 1563), sig. A.iiii.f.

Blundeville, the gentleman humanist, practical mathematician, friend of Edward Wright and his fellow East Anglian, reminded scholastic critics of his *Art of logike* in the vernacular that Greek and Latin had once been vernacular languages themselves.

Clients like Harriot also had to face attacks from fellow mathematical practitioners who, far from closing ranks against the purists and the disinterested, sought advantages over each other. I agree with Eric Ash when he argues that mathematical practitioners did not form a 'community'. They could be acutely aware of divisions created by different levels of education and social status. Harriot was one of three practitioners, along with John Dee and Thomas Digges, who were singled out as forming a mathematical elite.[41] They were not always singled out for respect: some of their more practical and less educated peers were unimpressed by their grasp of the practicalities of arts such as navigation. As we shall see later, Edward Wright recorded the same criticism.

Digges liked to invoke the classical story of Apelles the painter and his shoemaker critic, and used it to warn sailors to leave the principles of navigation to mathematicians. The unlearned fought back. Robert Norman famously complained to his patron William Borough that mechanicians and mariners like him were told by 'learned' mathematicians not to meddle in subjects such as magnetism and the longitude.[42]

It was for such reasons that learned mathematicians like Digges, Harriot and Wright were sensitive to criticism. All three used their brief if far from typical periods at sea to defuse the attacks of experienced seamen. In *Stratioticos* of 1579, Digges wrote that 'by Masters, Pilots and Mariners, I have been answered that my demonstrations were pretie devises, but if I had bene in sea service I should find all these my inventions mere toies'. As a result, he came to 'half distrust my demonstrations, and to thinke that reason did abuse me … To resolve myself of this paradox I spent a xv weeks in continual sea service'. Digges reported that his self-confidence was restored because the sea dogs agreed he was right: 'Sithens which time, I have learned no more to be abused by the opinions of men, what Office, or Degree soever they have born, or what fame soever go of them, if Reason be repugnant to their opinions.'

Digges ended *Stratioticos* by suggesting that continued criticism would make him wary about publishing in the future. It may even be that Momus-like comments were responsible for the loss to posterity of his 'Treatise of Greate Artillerie and Pyrotechnie'. He wanted encouragement to print it and, if he did not get it, then 'by the example of my Father, Pythagorically, I will content my selfe *per manus tradere*, and to communicate them only with a fewe selecte friendes'.

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41 Ash, *Power, knowledge and expertise*, pp. 182–5.

42 See, for example, T. Digges, in L. and T. Digges, *A prognostication everlasting of right good effect. The addition. A short discourse touchinge the variation of the compasse* (London, 1576); and R. Norman, *The newe attractive, containyng a short discourse of the magnes or lodestone* (London, 1581), sig. B–B^{v}.

He may have passed it from hand to hand, but never to a printer, and there is no extant manuscript.[43]

Mathematical practitioners also used their dedications to seek patronly protection from very personal problems. A fascinating example is the *Mathematicall jewell* of the self-styled gentleman mathematician and instrument designer John Blagrave. Blagrave's dedications repeatedly allude to a Jarndycean dispute over title to some family lands, and he suggested that the costly affair impeded the production of his work and books. He used his dedication to Burghley to thank him for his support during 'our late extreame & most iniurious vexation'. We learn of 'concealed knaues … who ioyning with a famous lewde pettifogger … [made up a] whole commonwealth of villainous trechery …', leading to a 'turmoile in law' which lasted six years. Consequently, *The mathematicall jewell* was delayed and Blagrave was 'forced to cut al the prints myself to my great paines and let of time'.[44]

However, the most devastating personal attacks concerned the intellectual probity of the author. A commonly mentioned charge was that the client was not seeking to advance the commonwealth (as all had to say that they did) but was promoting his private interests. The gift-giving culture of patronage suppressed hints of financial gain. This made it easy for a well-off gentleman like Thomas Digges to despise mathematical practitioners who 'stouped to filthy lucre', but difficult for a maker and seller of instruments like Robert Norman. In 1584 Norman sought, as did Tanner, Wright and others, the protective dedication of Charles Howard, soon to be appointed Lord High Admiral, for his translation and edition of Cornelius Antoniszoon's rutter, which he called the *Safeguard of sailors*. He went to great lengths to defend hydrography 'before any other art or science chiefly for the great and publick commoditie in general' and he denied the 'sinister constructions' that he was putting his own profit before that of his country.[45]

We come at last to accusations of plagiarism, the accusation actually levelled against Wright and his *Certaine errors*. This kind of attack upon an author's personal character was aired surprisingly often in the front matter of the period. It was clearly dishonourable to present another's work as one's own to one's advantage and the other's loss in the competition for patronage. There were many opportunities to make the charge, not least because of the scope for misunderstandings. As we saw, many works circulated for years in manuscript form, giving opportunity for bare-faced plagiarism.

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43 T. Digges, *Stratioticos*, sigs Aiv–Aiv.v, aiiii.

44 J. Blagrave, *The mathematicall jewel* (London, 1585), sig, ¶ii–¶ii.v.

45 For Digges on 'filthy lucre', see the 'Preface to the reader' in his *Pantometria* (London, 1571) and his *Alae seu scalae mathematicae* (London, 1573), sig. Aiiii; C. Antoniszoon, *The safegard of sailers, or great rutter. Translated out of Dutch into English, by R. Norman, hydrographer* (London, 1584), 'Epistle dedicatorie' and 'To the friendle readers, saliers and mariners'.

Again, in matters of intellectual credit and acclaim, the intrusion into the personal world of patronage and honour of a public print culture raised issues concerning intellectual property to which there were no agreed solutions in the early modern period. For example, many of the early books in the vernacular about mathematics and science were translations or collations of classical or foreign language texts, a fact often concealed on the title page and referred to only obliquely in the front matter. Were these plagiarisms? What distinguished Jean Taisnier's publication of Petrus Peregrinus's medieval '*Epistola de magnete*', often criticized as a plagiarism, from Richard Eden's version of it or, as we shall see, from Edward Wright's inclusion of material by Pedro Nunez?[46]

Intellectual property was also confused by the ability of patrons to be presented as authors. The most famous contemporary example was James I's 'authorship' of the King James Bible. In this context, consider Harriot's *Briefe and true report* of 1588, the only work that was printed during his lifetime. Sir Walter Ralegh was proprietorial about the knowledge Harriot developed whilst in his employ. The wordy title page of the *Briefe and true report* strongly suggested that just as Virginia was Ralegh's colony, so the *Report* was Ralegh's too. Ralegh's name appears in prominent capitals, in contrast to the concluding small italics which mention 'Thomas Harriot, *servant to the abovenamed Sir Walter*, member of the Colony, and there imployed in discovering'. In another blurring of what made an author, friends and collaborators willingly contributed material without expectation of acknowledgement.

These blurred boundaries created acrimonious plagiarism disputes and made it easy for Momuses to stir up suspicions. John Dee, in his *General and rare memorials pertayning to the perfect art of navigation* of 1577, claimed that the rumours that he was a conjuror had been begun by an 'impudent mechanician' who falsely claimed that Dee had stolen his design for a paradoxall compass. Dee demanded stronger protection from patrons before he would publish the remaining and 'perfecting' volumes.[47] A particularly vicious dispute occurred in the 1610s between two of Edward Wright's colleagues, William Barlow and Mark Ridley. They produced rival English versions of Gilbert's *De magnete*. According to Barlow, his *Magneticall advertisements* had been presented in manuscript in 1609 to his patron Sir Thomas Chaloner, master of the household for Henry, Prince of Wales. But he only got it into print in 1616, and he then complained that Ridley's

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[46] J. Taisnier, *Opusculum perpetua memoria dignissimum, de natura magnetis et ejus effectibus, Item de motu continuo* (Cologne, 1572); R. Eden, *A very necessarie and profitable booke concerning navigation* (London, 1579). Taisnier probably plagiarized the first printed edition, prepared by A.P. Gasser, *Petri Peregrini Maricurtensis De magnete seu Rota perpetui motus libellus* (Augsburg, 1558).

[47] See in the front matter to J. Dee, *General and rare memorials pertayning to the perfect arte of nauigation* (London, 1577): 'A necessary Aduertisement, by an vnknown freend, giuen to the modest, and godly Readers: who also carefully desire the prosperous State of the Common wealth, of this Brytish Kingdom, and the Politicall Securitie thereof.'

work of 1613, *Magneticall bodies and motions*, was a plagiarism. Ridley returned the allegation, thereby precipitating a backbiting pamphlet war worthy of Momus himself. In the process, Ridley got Edward Wright to admit that he had contributed without acknowledgement important sections of Gilbert's *De magnete*. Authorship and intellectual property were complex issues.[48]

With these issues of manuscript circulation, authorship, plagiarism and patronly support in mind, let us reconsider 'Arcticon' and why Harriot did not see into print the work which might have established him internationally as a navigation expert. The traditional and obvious explanation remains possible: Harriot was simply incapable of bringing 'Arcticon' or any other work to the level of completion that he desired and printers demanded. But in this chapter I have prepared the ground for an alternative explanation based on considerations of patronage, and I conclude by presenting the evidence in its favour, which is in part circumstantial and in part from the case of Wright's *Certaine errors*.

In the first place, there are good grounds for thinking that Harriot's patron Sir Walter Ralegh did not want 'Arcticon' to be printed. In patronizing Harriot, Ralegh had nurtured Harriot's outstanding expertise in order to enhance his colonial adventures. Why would Ralegh want the navigational edge that Harriot's training conferred on his captains to be publicly available to the captains of his rivals, both abroad and at home in Whitehall? His rivals included not just Spanish but also English adventurers and privateers. For example, Harriot pleaded with Robert Cecil on Ralegh's behalf to prevent the maps of 'Eldorado' (Guiana) in the possession of Ralegh's lieutenant Keymis from falling into the hands of rival courtiers.[49] This occurred in 1596, just as the ownership of Wright's research became controversial.

Moreover, Harriot claimed that he had produced a more detailed work on Virginia. Historians lament this lost 'chronicle' above all the others of Harriot's missing works, including 'Arcticon'. Shirley was of the opinion that it never appeared because Ralegh would not authorize its publication.[50] As Ralegh's stock declined in the 1590s and as Harriot sought new patrons, Ralegh could have helped him by allowing works like his Virginia chronicle and 'Arcticon' to be circulated and printed. If Ralegh was no longer the ideal, protective dedicatee, he could have allowed Harriot to dedicate it to someone else, most obviously his friend and successor as Harriot's patron, Henry Percy, the ninth Earl of Northumberland. We can speculate, but the fact remains that the chronicle and 'Arcticon' joined the lost works which never enhanced Harriot's reputation outside his immediate circle.

Likewise, we can only guess how closely Harriot's 'Arcticon' matched Edward Wright's *Certaine errors* at the pinnacle of Elizabethan mathematical navigation. However, there is evidence to address the issue of why *Certaine errors* came

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48 For details of the dispute, see S. Pumfrey, *Latitude and the magnetic Earth* (Duxford, 2002), pp. 203–12.

49 Shirley, *Thomas Harriot*, p. 231.

50 Ibid., p. 155.

to dominate the field and to secure fame for Wright, to Harriot's detriment. The fascinating publishing history of Wright's *Certaine errors* has been described before, but it acquires a much greater historical significance when it is contrasted with the fate of lost works like Harriot's 'Arcticon', Digges's 'Treatise of the art of navigation' or Dee's 'Astronomicall, & logisticall rules, and Canons'. It acquires significance once we realize the crucial role of patronage, specifically the patronage given to Wright by the Earl of Cumberland to facilitate the transition of *Certaine errors* from a private manuscript in the Cumberland circle to a printed book admired throughout Europe. What, then, differentiated the connections linking Harriot, his book and his patrons and those linking Wright, *Certaine errors* and Cumberland?

Wright's career paralleled Harriot's very closely until the publication of *Certaine errors* in 1599. They were born and also proceeded BA within a few months of each other, with Wright graduating from Caius College, Cambridge. To be sure, Wright stayed at on at Caius for some years, while Harriot immediately left St Mary Hall, Oxford for London. However, by 1585 both were no longer pursuing navigation theoretically in Oxbridge and instead were sailing out of London on life-threatening adventures.[51] While Harriot was exploring the colony of Virginia for Ralegh, Wright tells us that he was captaining the *Hope* in Sir Francis Drake's West Indian expedition. Wright must have met Harriot in June 1586, when Drake's ships arrived to evacuate Harriot and the other survivors of Ralegh's colonial experiment. Perhaps Wright was sailing under the same pseudonym, 'Captain Carelesse', which he used in 1589 when a captain of Cumberland's on an expedition to the Azores.[52]

Harriot moved from the patronage of Ralegh to Northumberland, but Wright remained with Cumberland at least until *Certaine errors* was printed in 1599. Cumberland sought to support an extravagant courtier's lifestyle through his large-scale privateering. At one stage he commanded three squadrons, and when he commissioned the *Scourge of malice* in 1594, which drew 600 tons and cost £6,000, it was the largest ship ever built for an individual Englishman. Costly failures, particularly his expedition to Puerto Rico in 1598, threatened Cumberland's fortune and his patronage, but he adapted successfully to new and less risky corporate ventures. In 1601 he became a founder investor of the East India Company, which bought his *Scourge* for £3,700 as one of its first

51 A.J. Apt, 'Wright, Edward (*bap.* 1561, *d.* 1615)', *Oxford dictionary of national biography*. Cf. J.J. Roche 'Harriot, Thomas (c. 1560–1621)', *Oxford dictionary of national biography*.

52 E. Wright, 'The Voyage of the right Ho. George Earle of Cumberl[and] to the Azores, &c', appended to Wright, *Certaine errors*. At sig. A, Wright refers to '*Captaine Edwarde Carelesse, alias, Wright*, who in S. Frauncis Drakes *West Indian* voiage was Captaine of the *Hope*'.

merchantmen.[53] Wright managed a similar transition. By the 1600s he was giving lectures on navigation in London for the East India Company for £50 a year and he became one of elite group of mathematical practitioners who gained the patronage of London's merchant adventurers and, later, that of Prince Henry and his circle. This circle overlapped with the merchants. It was while Wright was working on their joint colonial and commercial projects that he was able to dedicate the second edition of *Certaine errors* to Prince Henry. Henry's early death in 1612 was a major setback, because Wright was in line to become his librarian. Nevertheless, Wright was able to continue his work, and it was he who would bring Napier's work on logarithms to fruition. Wright was more fortunate than Harriot with his search for patrons, not least because Cumberland had secured his fame with the first edition of 1599.[54]

As a result of their months spent at sea in the 1580s, Harriot and Wright became concerned, as Dee and Digges had been in the 1570s, by the gulf between the rough and ready practices of most seamen and the high standards of accuracy which mathematicians knew were attainable through advanced techniques in navigation. Harriot and Wright both developed lectures, courses of instruction and manuals for their respective patrons in order to eliminate bad practices by sailors in the employ of the two rival privateers.

But was Wright any more eager than Harriot to get his manual into print? We have seen how several factors discouraged it. Clients were discouraged from the naked pursuit of their own advantage. Patrons might want to keep their clients' achievements to themselves and their circle if they were economically valuable. Moreover, as we saw, manuscript publishing was often considered more appropriate, especially when it permitted copies of a work to be personalized: for example, they could be adapted to particular navigations or navigators, as Dee had done for Richard Chancellor with his work on 'Astronomicall, & logisticall rules'.

Such norms surely applied to the work we know as *Certaine errors in navigation*. It took 10 years for the manuscript to come to press. We know this because on its very last page Wright introduced an appendix, his 'Voyage of the right Ho. George Earle of Cumberl. to the Azores, etc.' (this appendix concluded with Wright's remarkable map of the Azores, the Atlantic and the coasts of Spain, France and the British Isles, remarkable because it used a Mercator projection). Wright asked readers to remember his debt to Cumberland:

> by whom I was first moved, and received maintenance to divert my mathematicall studies, from a theoricall speculation in the Universitie, to the practicall demonstration of the use thereof in Navigation … the whole discourse of which

53 See P. Holmes, 'Clifford, George, third earl of Cumberland (1558–1605)', *Oxford dictionary of national biography.*

54 Roche, 'Harriot, Thomas'.

> voyage, *beeing the first occasion to me of writing the former treatise*, I thought good also as an appendix to adjoyne hereunto.[55]

In his dedication to Cumberland, Wright described the book as 'these first fruits of those my sea-labours'[56] and noted that 'the originall copy thereof … I had made, and presented unto your L. almost seaven years before'. Wright was counting back to 1589 from 1596, the year which saw the deaths of Cumberland's erstwhile pilot Abraham Kendall and Kendall's new master, Sir Francis Drake.[57] The precise circumstances which led to the form and fact of Wright's publication are recorded because Wright needed to explain and defend himself.

Just as Harriot had been employed by Ralegh, so Wright had been maintained by Cumberland for his expertise to give Cumberland's ships an advantage. Like Harriot, he had delivered improvements in the use of the cross-staff, solar declinations and charts and, some time before 1594, he discovered the geometrical secret behind Mercator's charts, presumably independently of Harriot. But where Mercator (and, it seems, Harriot) carefully guarded the secret, Wright was more open. He came to regret it. In *Certaine errors* he wrote that 'I wish I had beene as wise as he in keeping it more charily to my self'.

By 1594 Wright had communicated it to Thomas Blundeville, who explained in his printed *Exercises* that Mercator's maps were constructed according to a:

> rule I knowe not, vnlesse it be by such a Table, as my friende M. *Wright* of *Caius* colledge in *Cambridge* at my request sent me (I thanke him) not long since for that purpose, which Table with his consent, I haue here plainlie set downe together with the vse thereof.[58]

Three years later in 1597, Wright's future colleague William Barlow printed the actual method in his *Navigators supply*. Barlow noted that it was a demonstration 'which I obteined of a friende of mine of like profession vnto my selfe, euidently shewing the proportionall encreasing of those degrees, wherein consisteth the excellencie of that Carde'.[59] Wright's solution was now circulating more widely.

Wright had most reason to regret sharing it with Jodocus Hondius (Joost de Hondt) from the Low Countries. A Protestant, Hondius fled his native Ghent when it was captured by the Duke of Parma in 1583. He settled in London where he

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55 Wright, *Certaine errors*, sig. Q4 r–v.

56 Ibid., sig. A2r–v.

57 Ibid. The year is not, as previous commentators have assumed, 1592 or seven years before *Certaine errors* was published in 1599. Typical in this regard is R.V. Tooley, *Maps and map-makers* (London, 1987), p. 51.

58 T. Blundeville, *M. Blundevile his exercises containing sixe treatises* (London, 1594), pp. 326–8.

59 W. Barlow, *The navigators supply* (London, 1597), sig. I2. Barlow and Wright would later work together as clients at the court of Prince Henry.

developed a business as an engraver and cosmographer.[60] According to Wright, some time before Hondius left for the newly independent and Protestant Dutch Republic in 1593, he and his friends persuaded Wright that he could be trusted to 'have this booke for a few dayes to peruse: he also assuring me upon his faith and credit, that he would not publish it, or any part thereof without my knowledge and consent'.

Hondius broke his promise, mastered Wright's solution and in 1596/97 produced his famous 'Christian Knight' map of the world, one of the very first in print with Mercator's projection.[61] Wright reproduced apologetic letters written by Hondius to himself and to Henry Briggs. To Briggs, Hondius wrote that 'I would have published his whole booke for the common good, if I might have done it without breach of my faithful promise. And surely my conscience grudged to publish even this little which I have taken out: but the profit thereof moved me'. To Wright, he wrote lamely that 'I was purposed to have this set forth under your name: but I feared that you would be displeased therewith, because I have but rudely and without elegancie translated it into Latine'.[62] At least Hondius gave the map an entablature with greetings to '*Doctissimis Ornatissimisque viris D.D. R. Brewero, H. Briggio et Ed. Wrichto, medicis celeberrimis, Matheseos eximiis Professoribus*'.

Wright was understandably furious, not least because Hondius's deceit exposed him to the Momuses who 'may object that I do but *actũ agere*, in doing no more then hath been done alreadie by Gerardus Mercator, in his universall mappe many yeares since: and in publishing something already published by Jodocus Hondius'. Wright granted that his work was occasioned by 'that mappe of Mercator', but concerning the mathematics he insisted that 'how this should be done, I learned neither of Mercator nor any man els'. Certainly his map of the Azores and the coast of Western Europe predated that of Hondius. [63]

Although Wright vehemently defended *Certaine errors* from this serious suspicion of plagiarism, he also admitted borrowings when it was strategic to do so. He did so in order to defend himself from Momus-like fault finders, whilst at the same time refuting the charge that he himself was like Momus. The very title, *Certaine errors in navigation*, was redolent of the nitpickings of a scholarly mathematician, and Wright expressed concerns that experienced sea-dogs would oppose his reforms: 'it may be, I shall be blamed by some, as being to[o] busie a fault-finder my self'. He wanted to be seen more as 'a fault mender, then a fault finder'. He was therefore more than willing to own up to those who observed that

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60 A. McConnell, 'Hondius, Jodocus (1563–1612)', *Oxford dictionary of national biography*.

61 See Monmonier, *Rhumb lines and map wars*, p. 69.

62 Wright, *Certaine errors*, ¶¶ 4v– ¶¶¶r.

63 Ibid., ¶¶ 4v. A manuscript copy of Wright's map has been dated to c. 1595. See D.W. Waters, *The art of navigation in England in Elizabethan and early Stuart times* (London, 1958), pp. 550–51, plate 61.

'the errors I poynt at in the chart, have been heretofore poynted out by others, especially by Petrus Nonius, out of whom the most part of the first chapter of the Treatise is *almost worde for worde translated*'.[64]

However, the accusations that Wright had plagiarized small parts of his book from Pedro Nunez and Hondius were minor compared with the possibility that he had stolen the whole of *Certaine errors* from that other expert in navigation, Abraham Kendall. Kendall had served as a pilot for Cumberland and later for Francis Drake, with whom he sailed in the winter of 1595–96 on Drake's disastrous last raid. Fever swept through the fleet, killing one-fifth of the crew, including Drake and Kendall, his navigator. In his 'Praeface to the Reader', Wright recorded that:

> It is not unknowne to some of good place and reckoning, that one of the skilfullest navigators (as he was by many accounted) of our time and Nation, who died in Sir *Frauncis Drakes* last voyage, when he came to [the moment of death] gathered and bound togither into a bundell all his nauticall notes and observations, and [was] to have cast them into the sea.

A captain persuaded him that he should let others benefit from his expertise:

> Whereupon this great navigator drewe forth a booke out of his bosom, and delivered it unto this captaine not long before his death. This booke was shewed by the same Captaine to the R. Honourable [Charles Howard,] the L. high Admirall of England in the Cales voyage, as being made by that famous navigator, which his Lordship also (as it was reported) thought good should be perused and published. These news moved some expectation of that booke: so as the right Honourable, and my very good Lord the Earle of Cumberland, hearing of it, was desirous also to have a sight thereof, and remembred me unto that Captaine, as one not insufficient to peruse and correct the same. And hereupon the booke was brought unto his Lordship, at the time and place appointed at Westminster, and was there also delivered unto me, to be perused and corrected … [Wright inspected the book and] I found it everywhere to agree with mine, and to be a copie of the same booke worde for worde, which I made and presented unto his Lordship, almost seven years before, as the next morning it plainly appeared both to his Lordship and to the captaine himself that brought it, by comparing it in all poynts with the originall exemplar of the same booke, which I then brought unto his Lordship.[65]

It seems certain that the decision to print *Certaine errors* was taken only after it was discovered that the research conducted by Wright for Cumberland was going to press in the false belief that it represented work conducted by Kendall

[64] Ibid., ¶¶ 3v, emphasis added.

[65] Ibid., ¶¶4v–¶¶¶2.

for Drake. Wright still spent three more years refining it for publication, a refining process which he continued as he developed the even longer and more virtuosic second edition of 1610. Refinement probably explains the complexity of *Certaine errors* in the printed form that survives, a complexity criticized by some commentators.[66]

Having reviewed the incidents involving Blundeville, Hondius, Kendall and Barlow, we are in the unusual position of being able to see, in a particular example, the crucial functions in Elizabethan publication of patrons and letters of dedication to them. Wright's letter of dedication to Cumberland employs many of the conventions of the genre.[67] However, we soon realize that in this case many of the conventions encode specific historical facts.

Cumberland was indeed Wright's long-standing patron, which made Wright's dedication appropriate as well as advantageous: 'these first fruits of those my sea-labours, could not be more justly due to any, then to your self: as by whose beneficiall hand they have chiefly been cherished'.

Likewise, Wright's protestation that the work was not ready for publication was simultaneously conventional and true. Cumberland did indeed know best 'the causes that most moved me thus unseasonably (as it were) to pluck the same before time, that is, the publishing of part hereof already by one: and the stealing of an other part by a second man, and the daunger of publishing the whole by the third'. Wright noted how the Earl had confirmed that Wright had presented him with the same book seven years earlier.

Wright continued to express the conventional reluctance to publish work unworthy of his patron. He had tried to make 'supply of such wants, as were in that book', but time was against him, forcing him to 'acknowledg mine own [work] openly, with all faults'. Indeed, in another aspect both conventional and true, Wright was able to present publication not as his selfish desire but as Cumberland's suggestion. He had 'thought it best to follow your Lo. advise, rather by publishing it myself … then either to have it by peecemeale dismembred, or unjustly chalenged by some other man as his owne'. Given the complicated and

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66 I suggest that Wright took pains to transform his 'Certaine errors', a practical manuscript work for Cumberland's captains, into his printed *Certaine errors*, a public demonstration of his mathematical abilities to the reading public, to Cumberland and later to the Prince of Wales. The 1610 edition contained more and fuller proofs of his mathematical abilities. For example, the famous table of spacings of the parallels of latitude in Mercator's projection took up only 6 pages of the 1599 edition, but 23 pages in the 1610 edition, with levels of precision of 'little direct use to most readers', according to Monmonier, *Rhumb lines and map wards*, p. 65. Perhaps, then, in the absence of Wright's earlier version, Eric Ash was hasty to conclude that Wright wrote for mathematicians while Harriot wrote for sailors. A better explanation might be that Wright rewrote for his patron and fellow mathematicians, while Harriot never did. See Ash, *Power, knowledge and expertise*, pp. 165–76, esp. p. 172.

67 Wright, *Certaine errors*, sig. A2–A3.

contested background, Wright needed Cumberland's protection from his critics, and he closed by desiring 'your Lo. to vouchsafe the same [book] the safegarde of your honorable protection, both against these, and other injuries that may be expected of ignorant, or malicious tongues: as not knowing whome better to flie unto to be protected, both for your honourable favours towards me, and for your noble authoritie, joyned with no lesse skill, experience, and judgement in these matters belonging unto Navigation'. His references to malicious tongues were both tropical and true.[68]

By way of conclusion, let us pursue the reasonable conjecture that Harriot's book 'Arcticon' underwent a similar process of composition and circulation to Wright's unprinted manual. Let us assume that in the early 1580s Harriot began to assemble material for the instruction of the captains retained by his patron Sir Walter Ralegh. These were written instructions and step-by-step advice for Ralegh's sailors to read, mark and learn. Multiple copies would have been made because each captain or navigator needed a copy (perhaps a bespoke copy) and because Harriot would have continually updated and improved his advice, adding discoveries of his own, such as the correction for parallax in a cross-staff or the construction and use of Mercator-style maps. Since Ralegh provided Harriot with his livelihood, Ralegh regarded the content of these manuscript manuals as his private property. It was not in his interests for 'Arcticon' to be printed and sold at international book fairs, to be devoured by Spanish admirals and rival privateers. Nor was Harriot 'eager' to publish it, perhaps because he understood and was content with the nature of his patron–client relationship.

It would have been different if, as happened with Wright's work, discoveries in 'Arcticon' such as Harriot's solution to the Mercator problem had become public knowledge. The issue would no longer be one of preserving secrecy for competitive advantage. It would now be the protection of Ralegh's honour and discernment as a patron, and the public recognition of Harriot as his client, both established through an obsequious dedication. In the event, the secrets of Harriot's 'Arcticon' remained within Ralegh's circle. By the time the similar content of his competitor's *Certaine errors* was being talked about and recommended for publication by the Earl of Cumberland, Harriot was at a twofold disadvantage. First, following the failure of his Guiana expedition, Ralegh's credit as a courtier and patron had finally evaporated. Secondly, Harriot was no longer pursuing utilitarian work for Ralegh, but more philosophical studies for Northumberland the 'wizard earl', the new patron who would also fall from grace before long.

And so, we may presume, those few copies of 'Arcticon' that were not used to destruction at sea languished in Harriot's drawer. The norms of patronage and publishing in late Elizabethan England did not promote its journey to the printing press, for this was common in the era. It was eventually lost, but historians must realize how common this was: witness the similar mathematical works mentioned by Elizabethan practitioners such as Cunningham, Dee and

[68] All quotations from ibid., 'Epistle dedicatorie', sig. A2–A3.

Digges. It seems that Wright's *Certaine errors* was saved for the press and for posterity by the twin accidents of the accusations of plagiarism and the ability of the Earl of Cumberland to defuse them. In different circumstances we could have been celebrating Harriot and not Wright as early modern England's greatest innovator in the art of navigation.

Chapter 8

Last Act? 1618 and the Shaping of Sir Walter Ralegh's Reputation[1]

Mark Nicholls

The boy in Sir Walter Ralegh dies with his son. Through all the miseries and discomforts of his last voyage to Guiana, Ralegh's diary shines with delight and youthful exaggeration, glittering when all around is dark. When he fell sick, his fever was far worse than any survived by another man. He delighted in the beauty of 'Magelanns cloude … which riseth and setteth with the stares' and wondered at the great number of sea-birds on an island off the coast of Guiana. He did his best to understand the Atlantic weather. 'I observed this day', he wrote in mid-October, 'and so I did before, that the morning rainebow doth not give a faire day as in Ingland.' Rainbows predicted storms so frequently that late in October he half-believed 'the raine would never end'. Novelty piled on novelty. The rainbow he spotted off Trinidad made a 'perfait cirkell' in the sky, 'which I never saw before'.[2] There is a constant sense of challenge and competition, frustration with the shortcomings in others, never in himself. And then, in February 1618, news reached him that young Walter – Wat – had died in the botched and futile capture of San Thomé, a Spanish settlement on the Orinoco. His expedition, sent with great hopes upriver, had returned empty-handed. It had found no silver; it had cost him his eldest boy.

Instinctively, the bereaved man put pen to paper. 'My braines are broken', he wrote to his wife Bess in March 1618. 'As Sir Francis Drake and Sir John Hawkins died heart-broken when they failed of their enterprize, I could willinglie doe the like.' In this letter one sees the natural writer at work. Frantic and unbalanced, that lack of balance still has logic: the short, despairing essay is followed by a long, angry postscript, casting all the blame elsewhere. On his now dead lieutenant Lawrence Keymis, on his spineless men, on those at the English court – and of course he meant the king – who had conveyed all his plans to Spain. 'There was

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[1] I am most grateful to all those who commented on early versions of this chapter, including the one presented in the 2008 Thomas Harriot Lecture, and on the final chapters of Mark Nicholls and Penry Williams, *Sir Walter Raleigh. In life and legend* (London, 2011). My particular thanks go to Anna Beer, Allen Boyer, Pauline Croft and Larry Tise.

[2] BL Cotton MS Titus BVIII, ff. 169–74.

never poore man soe exposed to the slaughter as I was.'[3] His words are elegant and powerful, for when dwelling on disappointment Ralegh was writing his way through familiar territory. His true voice is heard most clearly. If self-obsession and the vulnerability of an unsuccessful military commander sometimes obscure grief, the grief is nevertheless obvious and far from artificial.

Great events shape great reputations. This chapter will explore the extent to which everything that happened to Ralegh after his disastrous Guiana expedition fashioned the ways in which people have remembered him across four centuries and in which they still regard him today. To clarify just what did happen, it will look first at the processes of law pursued against Ralegh in that year and at his final moments on the scaffold – in part through the eyes of an old and loyal friend, Thomas Harriot. It will then consider, briefly, some of the ways in which his reputation has developed over time.[4]

Ralegh endured three great crises in the course of his career. Those crises led in each case to disgrace and imprisonment in the Tower of London, but they were otherwise very different from each other. While an element of self-destruction is common to all three, the first crisis was entirely self-inflicted. Early in the 1590s, Ralegh secretly married one of the queen's ladies in waiting. Elizabeth – Bess – Throckmorton was a daughter of Elizabeth I's former ambassador in France, Sir Nicholas Throckmorton. She was a strong-minded, determined woman. Her brother Arthur, who was very fond of her, likened her to the dark, driven force of Arthurian myth, referring to her privately as 'Morgan le fay' in his diary.[5] Happily, Bess was a match for her husband in every sense and the early affection between them persisted, even strengthened, as the years passed. It was very soon tested: Elizabeth learnt of the marriage only after the birth of their first child and reacted with measured fury against this act of *lese majesté*. Since neither Ralegh nor Bess showed any contrition, the queen sent them both to the Tower.

They were released after a few months – their son died young, Ralegh proved useful in administering the rich cargo of a captured Portuguese carrack landed in the south-west and Bess kept uncharacteristically quiet. But things thereafter were never the same. Ralegh was now 40, on the verge of middle age, and his unfulfilled ambitions depended on a queen who no longer entirely trusted him. While he possessed estates in Dorset and Munster, status as Lord Lieutenant of Cornwall, and a secure income from monopolies and other sources, his court career never fully recovered. He was no longer Elizabeth's 'oracle'.[6] He was reminded, and not

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3 A. Latham and J. Youings (eds), *The letters of Sir Walter Raleigh* (Exeter, 1999), pp. 353–5.

4 See A. Beer, *Sir Walter Raleigh and his readers in the seventeenth century. Speaking to the people* (Basingstoke, 1997).

5 A.L. Rowse, *Raleigh and the Throckmortons* (London, 1962), p. 276.

6 R. Naunton, *Fragmenta regalia, or, observations on the late Queen Elizabeth* (London, 1641), p. 31.

for the last time, that status and indispensability were useful bulwarks for those out of favour.

The second crisis of Ralegh's career had its origins in another fundamental political miscalculation. By the end of the sixteenth century, he had confirmed a partial revival in his fortunes through friendship with the father and son who stood at the centre of late Elizabethan political life, William and Robert Cecil. He wrote candidly to both, commiserated with them in their losses and celebrated with them as Robert advanced in the world. After William Cecil died in 1598, the alliance with Robert continued. An increasingly paranoid Earl of Essex lumped them together as his principal enemies, and the younger Cecil entrusted his only son to Ralegh's care at Sherborne during the summer of 1600. But then the friendship turned sour. Ralegh may have become frustrated at Elizabeth's continued reluctance to make him a privy counsellor; it seems that he blamed Cecil rather than the queen for that frustration. Cecil felt nervous and vulnerable at the prospect of a Stuart succession and began to regard some of his friends as liabilities. He was always civil to Ralegh's face. Like his father, Cecil seldom expressed his true feelings, preferring to obscure meaning in elaboration and pointed allusion. However, it was obvious from what he did say that he felt abandoned by a one-time ally.

So Robert Cecil made new friends, and those friends pursued their own political agendas. Over the next two years Ralegh was repeatedly disparaged in a series of letters from Henry Howard, the crypto-Catholic youngest brother of the late Duke of Norfolk, written to James VI to assure him of Cecil's (and Howard's) loyalty. Howard's criticism was intemperate and unrelenting. Ralegh, he wrote, was indiscreet, incompetent and hostile to the very idea of James's succession to the English throne. Cecil's break with Ralegh had drastic consequences, but the collapse of their friendship cannot be blamed on the conscious machinations of either man. It is more accurate to suggest that their deteriorating relationship demonstrates an age-old tension in political life: strategic friendships cannot always bear the burden of expectation, and the close proximity of a court magnifies every slight and erodes trust.

The consequences that follow Elizabeth's death in 1603 now seem almost inevitable. Like many other courtiers, Ralegh rushed off to meet the new king, but James made no attempt to hide his wariness. John Aubrey records an atrocious pun, which may well be authentic: 'On my soule, Mon', said James, 'I have heard rawly of thee.'[7] Here was no basis for trust or confidence. In the space of a few traumatic weeks, Ralegh lost his monopolies, a crucial source of income, and his captaincy of the guard. He was also told, at very short notice, to give up his tenancy of a prestigious London residence, Durham House on the Strand. Worse was to follow. On 15 July he was detained for questioning upon suspicion of high treason. Implicated by his friend Lord Cobham in the so-called Main Plot, ostensibly a scheme to foment a Spanish invasion and to topple James from his

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[7] A. Clark (ed.), *'Brief lives', chiefly of contemporaries, set down by John Aubrey* (Oxford, 1898), vol. 2, p. 186.

throne in favour of his cousin Arbella Stuart, Ralegh was conveyed to the Tower and then tried on a charge of high treason at Winchester on 17 November.

There are moments in the history of England which define theatre, and few fictional courtroom dramas come close to matching the cut and thrust between accusers and accused, the personal crises and the unexpected shifts of fortune played out in the Great Hall of Winchester Castle that day. Many spectators paid handsomely for their seats, and they got their money's worth. Ralegh was dignified, firm in maintaining his innocence, while the Attorney General, Sir Edward Coke, lost his temper and lapsed at times into incoherence. For all Coke's failings, however, the drama lacked an unexpected denouement. A guilty verdict was returned, and the much-maligned jury had little alternative, not because they were subjected to pressure from above, but rather because Ralegh was a guilty man. As the law stood, thoughtless and ill-advised remarks could sustain a conviction for treason, and Cobham's subsequent testimony made clear that Ralegh's words had betrayed him on at least one occasion. At some point in May or early June 1603, Ralegh had come away from the court angry and frustrated, and in his rage he had urged Cobham to negotiate with the Flemish ambassador 'that he should doe best to advertise and advise the king of Spaine to send an armie against England to Milford Haven'. The reported words capture Ralegh's belief in prompt action: 'many more', he had growled, 'ha[ve] been hanged for words then for dedes'.[8] Cobham's testimony was not without its problems, and as Ralegh never tired of pointing out, his former friend was to all intents and purposes the only prosecution witness. Nevertheless, this does not mean that Cobham, in incriminating himself, was not telling the truth.

The succession of James I in 1603 had been foreshadowed by all sorts of concerns and fears.[9] Everything had in the event passed off peacefully, but even in November 1603 many disturbing questions remained unanswered. How would the English court react to a Scottish king and his Scottish entourage? Were all the English nobility and gentry really happy with the new dispensation? Xenophobia was widespread and not particularly well concealed.[10] In such times one should not be too surprised when sacrificial victims – expendable men and women belonging to no particular alliance and with no particular following – are offered up. Political bloodletting somehow reduces tension. It redresses the anticipated balance so important to an early modern view of the world: when some rise, others must fall. This bloodletting may not be orchestrated, but it is remarkable how often one sees something similar when a dynasty changes, a leader goes and a generation passes. An instinctive process is of course facilitated when the

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8 The National Archives, State Papers [SP] 14/4/91.

9 See C. Lee, *1603. A turning point in British history* (London, 2003); and D. Newton, *The making of the Jacobean regime. James VI and I and the government of England, 1603–1605* (Woodbridge, 2005).

10 J. Wormald, 'Gunpowder, treason and Scots', *Journal of British studies*, 24 (1985), 141–68.

victims contribute through folly to their own destruction. All the political nation has to do is sit back, watch the sufferings of others and be grateful that the blow has fallen somewhere else. Ralegh was probably not brought down in 1603 as a result of Cecil's scheming, but Cecil, like everyone else in court, did not try very hard to save him. The uncertainties of the new Jacobean world simply did not allow scope for any generous gesture or show of support.[11]

Something new, however, emerged from events at Winchester. Quite unexpectedly, Ralegh transformed himself from an upstart villain, too smart by half, into a popular hero. His behaviour during his trial was impeccable, while his prosecutors blundered through a series of gaffes and miscalculations. Continuing doubts over Cobham's evidence prompted James to spare Ralegh's life, and indeed he spared Cobham too. But that was as far as his mercy extended. Both men remained prisoners in the Tower. Ralegh was there for 13 years, until his remaining friends and allies persuaded a sceptical king, desperate for money, that there was treasure to be found in Guiana and that Ralegh was the man to find it.

I

This leads us to consider the third grand crisis, and the last. When the sea captain came home empty-handed from South America, the king was confronted with a problem. How should he respond to Ralegh's failure and to the anger that the attack on San Thomé had aroused in Madrid? This was an unwanted dilemma. The European situation was difficult, war was looming in Germany, and the king pinned a great deal on maintaining his diplomatic triumph of 14 years standing: the peace with Spain. Spanish diplomats were of course exerting every form of pressure open to them; they demanded that the 'pirate' be hanged or even sent to Spain for execution. Ralegh could not bring himself to help his king. He had the chance to run away – in Plymouth he abandoned a plan to sail for France at a very late stage – and had he taken that chance, James would have had his excuse for closing the discussion. But Ralegh miscalculated; his political judgement was never very sound. Fretting about the fate of his family, he could not quite see the personal risk that he ran by staying in England. He therefore remained under arrest, much to everyone's frustration in London.

We think that we understand – pretty much – what happened to Ralegh thereafter. Writers who have looked closely at these events, among them Edward Edwards, S.R. Gardiner, V.T. Harlow and recently Paul Hyland, have reached similar conclusions. Pursued by a vengeful king and a hostile or at least disinterested Privy Council, a sense of inevitability hangs over the legal process that summer and autumn. But maybe the picture is not quite so simple. There is some reason to suppose that James paused before identifying the course that he

[11] M. Nicholls, 'Sir Walter Raleigh's treason: a prosecution document', *English historical review*, 110 (1995), 902–24.

favoured and that at one point he contemplated mercy. Moreover, it seems likely that James's council were slow to unite behind him when he did, finally, decide to execute his elderly prisoner.

As with so many other aspects of Ralegh's political career, it is important to cut through the many layers of sympathetic interpretation and ask, first of all, if the process followed by James's government was legally robust. The answer, of course, is that it was. Ralegh still stood under sentence of death following his conviction for treason in 1603, and his recent actions had done nothing to mitigate his offences. By any impartial assessment, he had failed to honour promises and had shown a measure of disloyalty to the king who had set him free. Evidence for this double dealing and chicanery accumulated rapidly. When an investigatory commission questioned him in August 1618, they also put questions to members of his crew and to some of his fleet captains and other officers. The answers given by Roger North, John Chudleigh, William Herbert and others, while confirming their understanding that Ralegh had believed in the existence of a silver mine in Guiana, also conveyed a strong suspicion that he had been in league with France.

The evidence that they all gave, while too often based on hearsay and guesswork, was critical to the case. The commissioners for their part were convinced that the story of the mine had been spurious – the failure of the voyage had been proof of this – but they initially had some doubts over the importance of the negotiations with France. Testimony from the captains failed to allay their suspicions on the first point, but helped to persuade them that the French intrigues had in fact been significant. Ralegh was questioned several times, first – formally – by members of the council, then – informally – by Sir Thomas Wilson, Keeper of the State Papers, acting under a council commission.[12] The weaknesses of seventeenth-century interrogation techniques are here rather obvious: cross-examination is a laborious process, counsellors have other more pressing tasks to pursue and the skills of the inquisitor, intermittently used, grow rusty.[13] In 1618 the council ended up frustrated. They clearly believed that Ralegh was withholding important information, that he was toying with them, and there is reason to believe that they were right. At one moment Ralegh would hint that he had more to tell, particularly on his links with France. Next he would suggest that such details were trivial, adding that the king would hardly consider pardoning a man who gave evidence simply in the hope of avoiding execution.[14]

The situation called for a different approach, and here the investigators for once applied some subtlety. James let it be known that he would listen to what

12 SP 14/99/3, letter from John Pory to Dudley Carleton, 5 September 1618; 14/99/7, Wilson's commission.

13 See M. Nicholls, 'Strategy and motivation in the Gunpowder Plot', *Historical journal*, 50 (2007), 787–807 (804–6).

14 SP 14/99/10i.

Ralegh had to say, provided that the prisoner gave him facts and details.[15] This move was, no doubt, sincere, but the gesture also acknowledged that Ralegh, too, was desperate, that for all his 'wit' he was in a wretched state, broken, bereaved and prepared to grasp at any olive branch. The suggestion that transparency might result in a measure of clemency – that the king, for all his suspicions, was prepared to listen – drew him into candour. He now wrote to James more than once, giving new details, laying open every secret according to the jubilant Wilson.[16] But his notoriously poor political judgement betrayed him one last time. This candour included details of significant negotiations with the French court. He admitted, for example, that he had received a commission from the Duc de Montmorency, Admiral of France, and that he had been assured that the French ambassador would favour him with his letters to the Admiral.[17] England was not, of course, at war with France, but the admissions dismayed James and seem to have edged him decisively away from mercy. Understandably, he found it hard to see anything other than disloyalty in these subterfuges. Worse still, every admission of this kind contradicted earlier denials made under oath and left the king and his counsellors convinced that they were dealing with a 'dissembler'.[18] Such clues as there are suggest that James, at this stage, finally lost patience and resolved upon Ralegh's death.

It is important to be fair on the prisoner, to understand the awful situation in which he found himself. In these circumstances, miscalculation was understandable. Denied access to family, friends and trusted servants, Ralegh could only draw on inner fortitude. He took some perfunctory solace in his 'chymicall stuffes' or at least he carried his stills and bottles with him as he was moved from cell to cell within the Tower.[19] The memory of events long past somehow reassured him; he blamed the 'injustice' of his trial in 1603 on the recently disgraced Earl of Suffolk and on the dead Earl of Northampton (his old enemy Henry Howard). It was easier, if less accurate, to blame a corpse than a king.[20] Surely there could now be no repeat of such a miscarriage? He asked himself how an honourable man might respond to adversity and found his answer in classical history. Courageous Greeks and Romans, he noted, would take their own lives rather than endure the

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15 SP 14/99/21, Naunton to Wilson, 16 September 1618; 14/99/25, Wilson to Naunton, 17 September.

16 SP 14/99/48, 69, Wilson to the king, 18 and 24 September 1618. Ralegh seems to have written three letters to James at around this point. Only the last in the sequence, dated 4 October 1618 in the surviving copy, is extant. See Latham and Youings (eds), *The letters of Sir Walter Raleigh*, p. 375.

17 Latham and Youings (eds), *The letters of Sir Walter Raleigh*, p. 374. The letter of 4 October appears to have responded more candidly to interrogatories put to Ralegh some days earlier (SP 14/99/71).

18 SP 14/99/72, 73.

19 V.T. Harlow, *Raleigh's last voyage* (London, 1932), p. 271.

20 SP 14/99/10i.

humiliation of public execution. The father of his friend and fellow prisoner the Earl of Northumberland, he recalled, had shot himself in 1585 while a prisoner in the Tower. There was courage in choosing the moment of one's end, and if the act was unchristian, the outcome cheated predators and was bleakly satisfactory.[21] Northumberland's end and other tales from the history of the Tower, all blood and suffering, were never far from his mind.

The old Ralegh is still present in the autumn of 1618; he remains witty, vain and full of self-pity, but the wit now takes on an increasingly grim tone. He told Wilson that before being consigned to the Tower he had combed his hair for an hour every day. But the time for vanity was now past. What, he asked, was the point of fussing over his hair if the hangman would soon claim his head?[22] A little of the confident, almost childlike self-belief remained, as he emphasized his own abilities, notably in developing a process to distil sea water.[23] But the coping strategies extended to a contemplation of execution too; he was constant in his determination to 'die in the light', arguing the point through in his mind. If the Romans chose suicide, he would use a weapon from his own armoury. He would leave this world, if he could, on a public stage, speaking out against those who had brought him to the scaffold.[24] The silence of the suicide, however noble, would have to be set aside. English tradition would safeguard against the threat of a hugger-mugger execution, away from his all-important crowd.

How is it possible to justify an assertion that the Privy Council and judges did not entirely countenance James's desire for blood? There is, to start with, the insistence on legality, which led to a difference of opinion with the king over due process. In October 1618, the commissioners investigating Ralegh's case reported their findings to James in a paper drawn up by Ralegh's old adversary Sir Edward Coke, the man who had led the prosecution so ineffectively in 1603. This is a strange document, subservient to royal commands yet subtly objective and principled. It begins by making the significant legal point: Ralegh stands attainted of high treason, 'which is the highest and last work of law'. He is legally dead already and cannot be charged with crimes committed since. A trial of new offences is therefore redundant. This being the case, it is perfectly proper to proceed to execution without delay.

At that point, though, the commissioners drew back. This argument might carry weight as a point of law, but as a process of justice it was simply too arbitrary. They recommended that Ralegh should instead be called before the council, principal judges and some independent witnesses in the formal surroundings of the council chamber. There the king's counsel could proceed against him on charges centred around 'his acts of hostility, depredation, abuse as well of your Majesty's commission as of your subjects under his charge, impostures, attempt

21 Harlow, *Raleigh's last voyage*, p. 270.

22 *Calendar of State Papers, Domestic Series, 1611–18*, p. 575.

23 SP 14/99/96ii.

24 See SP 14/99/77, Wilson's report of conversations late in September.

of escape, and other his misdemeanors'. The focus was on new offences and, consequently, on a fresh legal process, notwithstanding the conviction for treason. The commissioners suggested that Ralegh should be heard, that he should face witnesses 'if need be', a pointed concession given that he had begged in vain for just one witness to be produced against him at his trial in 1603. If no sentence could be passed down on a man legally dead, there should still be some judgment, or rather some public process of assessment. The lords and judges would therefore advise the king 'whether in respect of these subsequent offences, upon the whole matter, your Majesty if you so please, may not with justice and honour give warrant for his execution upon his attainder'. All this should be recorded in 'a solemn act of council … with a memorial of the whole presence'. Moreover, 'the heads of the matter, together with the principal examinations touching the same', should be given to members of the council 'that they may be perfectly informed of the true state of the case, and give their advice accordingly'. Given that the legal argument was so clear, these formalities strike the reader as a salve to the collective conscience.[25] Again, thoughts – ours and theirs – go back to Winchester. The king's decision to spare Ralegh from the gallows in 1603 has taken on, after 15 years, a legal significance that runs beyond personal clemency. The commissioners suggest that it cannot now be set aside arbitrarily, without affecting both English justice and the king's honour.

Let us be clear about what the commissioners were saying. These recommendations indicate their belief that to refuse Ralegh a hearing – and one that came as close as possible to a fresh trial without rejecting the force of a conviction for high treason – would be to deny him justice. The commissioners' stand on the principle of a fair hearing deserves respect. It is more than legal pedantry, if less than an open challenge to the king. Public opinion seemed to pick up the vigour of their argument. Rumours in London suggested that Ralegh might soon be set free, that he 'had the libertie of the Towre'. One senses in these autumnal days a confidence not limited to the ordinary citizen.[26]

James, however, was not prepared to accept the recommendations. While conceding that an immediate execution without further process might be seen as too arbitrary, he rejected any public or semi-public hearing. The king also dwelt on the past. There was, he wrote, a risk that the commissioners' proposals would make Ralegh 'too popular, as was found by experiment at the arraignment at Winchester, where by his wit he turned the hatred of men into compassion for him'. It would, he believed, be more prudent to follow another course. Ralegh should be summoned to appear before the commissioners and no one else, just 'those who have been the examiners of him hitherto'. He would then receive the rudiments of a fresh hearing. Examinations would be read out, the Attorney General and

[25] Harlow, *Raleigh's last voyage*, pp. 295–6.

[26] N.E. McClure (ed.), *The letters of John Chamberlain* (Philadelphia, 1939), vol. 2, p. 173.

Solicitor General would inform against him, Ralegh might speak and, if necessary, ‘others’ might be ‘confronted with him, who were with him in this action’.

The king’s conclusion, though, was uncompromising. His commissioners envisaged a process of law, with at least the show of impartiality. The council and judges would advise the king whether he might proceed to execute the attainder. But James did not really seek advice on this matter. He had already determined the outcome. ’And then’, he wrote, ‘after the sentence for his execution which hath been thus long suspended, a declaration [shall] be presently put forth in print, a warrant being sent down for us to sign for his execution.’ Instead of a trial, he envisaged a ‘declaration’.[27]

There was little scope for further argument. Setting any remaining reservations to one side, the council followed the king’s wishes and summoned Ralegh before them on 22 October. Henry Yelverton, the Attorney General, accused him of falsehood and disloyalty. ‘Not weary of his fault, but of his restraint of liberty’, Ralegh had given ‘promise of a golden mine’, knowing or suspecting that the promise was worthless. Worse still, he had planned to stir up war between England and Spain. Ultimately, he had betrayed his king.[28]

Again, Ralegh was to all intents and purposes guilty as charged. He had gambled everything for liberty and – consciously or unconsciously –in so doing he had put to one side his own doubts as to the value of workable gold and silver deposits in Guiana. The commissioners held Ralegh, as commander of the expedition, accountable for the actions of subordinates, and the evidence upholds their decision.

Yet some people still had reservations. When, in a further effort to emphasize proper formalities, the sentence of death was confirmed in Ralegh’s presence by Sir Henry Montagu, Lord Chief Justice at the highest criminal court in England, the Court of King’s Bench, on 28 October, Montagu himself spoke courageously, declaring his own belief in the prisoner’s good character, suggesting that Ralegh possessed the fortitude to prepare for his end and praising his *History of the world*, an ‘admirable work’ that bore witness to its author’s Christian beliefs. There is more than a hint here of criticism directed against a king who insisted on severity. Montagu was an upwardly mobile, loyal careerist, but he was also a religious man and he seems to have sensed the dubious morality behind all this ostentatious due process. ‘You must do’, he said, ‘as that valiant captain did, who perceiving himself in danger, said in defiance of death, *death thou expectest me, but maugre thy spite I expect thee.*’[29] His emphasis fell on spite, with all the subtleties inherent in the word. Was the king now being more spiteful than death itself?

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27 Harlow, *Raleigh’s last voyage*, pp. 296–7.

28 Ibid., pp. 297–300, transcribed from BL Lansdowne MS 142, f. 396.

29 Ibid., p. 304.

II

The scene shifts to a scaffold erected in Old Palace Yard, Westminster, early on the morning of 29 October 1618. Granted his wish to die in the light, Ralegh met death with the courage that never failed him in difficult situations. His execution coincided with the Lord Mayor's Day in the city, and the authorities might have expected, or hoped, that this competing attraction a few miles downriver would reduce the crowds in search of sensation and spectacle. But the crowds turned out anyway; Ralegh was not to be denied his audience. Helped by a 'noate of remembrance', he addressed them for 45 minutes.[30] His speech survives in several different forms, forms which in their variety demonstrate the emotional engagement of an audience. The sequence in which he presented the several points made during his speech is preserved in Thomas Harriot's own, methodical note of the occasion, for Harriot stood watching in the throng.[31] This note is crucial to our understanding of the manner in which his old friend and patron passed out of this world. We overlook too readily the reasons why Harriot's company was so congenial to eminent men and women. He had a fine brain, of course, but he also seems to have been extremely sociable. The sober black suit that he favoured can leave the wrong impression. He crops up time and again at the dinner table and in the intimacy of the study, with Northumberland at Syon, with Ralegh in Durham House and in the Tower. This sociability apparently bred affection on both sides. Harriot was no fair-weather friend; he was loyal to both Ralegh and Northumberland through the worst of their misfortunes, and his presence in Old Palace Yard that October morning tells us a little more about a fascinating man.

In all these accounts and relations, several details are consistent. Much of the speech was predictable. Ralegh asserted that his expedition should be taken at face value. He died insisting that he had not plotted with France and that he had never contemplated a refuge abroad – both statements were of course false. With one eye on his future reputation, Ralegh laboured over many minor and rather obvious shortcomings, so much so indeed that, as Steven May suggests, the speech lacks proportion, absorbed as it is on the 'immediate' and the 'trifling'.[32] In character, but against convention, there was small measure of charity. Ralegh had only contempt for those he saw as false or weak, and he made no effort to forgive poor

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[30] *The arraignment and conviction of Sr Walter Rawleigh, at the King's Bench-barre at Winchester ... coppied by Sir Tho: Overbury* (London, 1648), p. 31; Beer, *Raleigh and his readers in the seventeenth century*, p. 88; McClure, *The letters of John Chamberlain*, vol. 2, p. 176.

[31] Reproduced in J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), p. 447. Of the various surviving versions of this long speech, the most reliable is perhaps that printed by R.H. Bowers, 'Raleigh's last speech: the "Elms" document', *Review of English studies*, new ser. 2 (1951), 209–16.

[32] S.W. May, *Sir Walter Raleigh* (Boston, 1989), p. 122.

Lawrence Keymis, who in his opinion had betrayed him through incompetence on the Orinoco.

Other aspects of the speech are more interesting. Here was a man pursuing his own agenda. 'Going to and fro upon the Scaffold, on every side he prayed the Company to pray to God to assist him and strengthen him'.[33] Ralegh took one expected norm of the execution ground and stretched it to a theatrical extreme. The plea for prayers was vigorous, almost threatening, there is anger here, and perhaps the gathering of courage, clamour and noise designed to close out reality. Most contemporaries saw only piety and resolution, but some caught the underlying fury. As Francis Osborne wrote four decades later, Ralegh's 'death was by him managed with so high and religious a resolution, as if a Roman had acted a Christian, or rather a Christian a Roman'.[34] One wonders which, and 'managed' is exactly the right word. There was no confession of great crimes, no significant apology to the king. He drew attention to the execution of the Earl of Essex in February 1601, ostensibly because some still blamed him for the Earl's death, but equally, perhaps, because he wished people to distinguish between a guilty man's pious search for forgiveness and an innocent man's stand on the principles of justice. Scaffold crowds in the early seventeenth century knew what to expect from the final speech of a condemned man, and they drew conclusions from the omissions. Departing cleverly from a norm, Ralegh committed the scene to public memory.

Public memorialization was what this was all about. When the axe fell, a 'muttring went through the multitude never died a braver spirritt'.[35] Someone shouted that the country had 'not such another head to cut off'. [36] 'They say', said Lewis Stucley, another acquaintance criticized by Ralegh on the scaffold, that 'he died like a Souldier and a Saint, and therefore then to be beleeved, not only against me, but against the attestation of the State.'[37] Ralegh's denial of guilt and his constant equanimity suggested to many that a defenceless prisoner had fallen victim to an arbitrary king and his feeble, misguided counsellors. Even the Spanish agent grudgingly acknowledged Ralegh's courage; he had been 'a person of great parts and experience, subtle, crafty, ingenious, and brave enough for anything'. 'The death of this man', wrote the agent, 'has produced a great commotion and fear here.'[38] In deciding how to deal with Ralegh, the Jacobean regime had faced a complex political conundrum: they had sought to go forward in accordance with the law, while also proceeding 'handsomely', as Edward Harwood had put

[33] Harlow, *Raleigh's last voyage*, p. 310.

[34] F. Osborne, *Historical memoires* (London, 1673), vol. 2, p. 477.

[35] Quoted in Beer, *Raleigh and his readers*, p. 96.

[36] Harlow, *Raleigh's last voyage*, p. 310; P. Hyland, *Raleigh's last journey* (London, 2003), p. 214.

[37] Quoted in Beer, *Raleigh and his readers*, p. 92.

[38] Harlow, *Raleigh's last voyage*, p. 315.

it early in October.[39] The complexity defeated them. Caught between irritation at the prisoner and his offences and a genuine concern for the dignity of due process, they had succeeded only in damaging the honour of their king.

III

Why should the manner of Ralegh's death remain significant four centuries later? What can it tell his biographer? In death as in life, of course, he remains interesting. Adults recognize his name, and children's publishers still turn out books for new generations detailing his career.[40] It is still hard to argue with the conclusions reached by A.L. Rowse half a century ago: Ralegh's name retains a power 'to compel the imagination of the English public, indeed of English-speaking people across the world, in America as much as in Britain'.[41] An answer to these difficult questions gradually emerges when considering the influences that have shaped that compulsion over time, out of the reasons for remembering. Ralegh's posthumous career is in many ways as interesting as his remarkable, eventful life. In different periods his ghost has been revered as a republican hero, studied as a bestselling author, honoured as a virtuous soldier, respected as a family man, admired as an Elizabethan sea-dog and founder of empires, mocked for his courtly excesses and his addiction to tobacco, and set aside as a curiosity, a Renaissance figure far too clever for his own good. This variety has arisen from the juxtaposition of enduring 'memories', all associated with one of the three crises discussed earlier. First, there is the dashing courtier and favourite who establishes colonies, smokes his pipe, spreads his cloak across a 'plashy place', flatters the queen and dallies so dangerously with her maids. Secondly, one recalls the steadfast prisoner at the bar in 1603 and thereafter the brilliant writer at work in his prison cell. Thirdly, there are the events of 1618. Each of these scenes still has power to 'compel the imagination', but despite the dash and drama of the cloak and the courtroom, it is perhaps the memory of 1618, the last act, that reveals Ralegh's character most closely. Nothing else has done so much to foster an ongoing, scholarly engagement with his career, and nothing else now goes so far in according him his rightful, nuanced place in British history.

Of course, the other crises have a historical durability of their own. Events surrounding the exposure of his clandestine marriage in 1592 offer plenty of material for popular literature.[42] A love triangle involving two powerful women

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39 SP 14/103/14, letter dated 3 October 1618.

40 For example, S. McCarthy, *Sir Walter Raleigh* (Oxford, 2002); C. Catling, *Sir Walter Raleigh, 1552–1618* (London, 2003); S.S. McPherson, *Sir Walter Raleigh* (Minneapolis, 2005).

41 Rowse, *Raleigh and the Throckmortons*, p. v.

42 There are several examples. It almost seems unfair to single out W.A. Devereaux, *Raleigh, a romance of Elizabeth's court* (Philadelphia, PA, 1910).

and a charismatic leading man captivates any audience in search of romance. But the Ralegh who emerges from these tales is usually a simple, unconvincing figure, stripped of historical credibility. He is the Sir Walter of Aubrey's raw tales, the ravisher of scarcely reluctant maidens, simplified and essentially weak, for all his swagger and glamour. And this fashioning of an Elizabethan courtier lies outside history in other ways. By the standards of the late Tudor elite, Ralegh was no womanizer. Alice Gould in Ireland, his wife Bess Throckmorton and a couple of ladies at court linked to him in gossip or later association: the list of conquests is hardly impressive for a lusty Elizabethan.

Even on screen, the courtier's colourful legacy is not as compelling as one might suppose. Hollywood epics have never really captured the complexity of the man. In *The Private Lives of Elizabeth and Essex*, Vincent Price played him as a sinister and slightly effeminate minion, in a minor role within a film dominated by Bette Davis's gloomy queen and Errol Flynn's frustrated Earl of Essex. In 1955, Richard Todd as a nice but really rather dim Ralegh fell for Joan Collins as Bess but failed to energize Davis's return to the role of Elizabeth in Henry Koster's *The Virgin Queen*. Clive Owen in the disappointing *Elizabeth: The Golden Age* (2007) does no better; he is all tobacco and potatoes, understandably lured by Abbie Cornish and, like Todd before him, nonsensically striving to leave the court, the source of favour and advancement, to set up colonies in the new world. These films make no attempt to reach beyond the box-office power of a name familiar to many, but in fairness nothing more is required. The American association and the romance with Bess suffice in screenplays driven by other priorities. Cinematic treatments of the Elizabethan court are always obliged to concentrate on the splendid, complex figure of Elizabeth herself.[43]

The events of 1603 offer an altogether more potent legacy. Lawyers in Britain and America still remember Ralegh's trial; the US Supreme Court took its lessons to heart as recently as 2004 in a reflection on the Sixth Amendment to the Constitution.[44] The proceedings at Winchester remain central to any discussion of some basic rights enjoyed by an accused person: the right to be confronted by more than one hostile witness, to understand the indictment and to enter the court on a level footing with one's prosecutors. Events during the trial fashioned Ralegh overnight into the courageous victim, the 'innocent' man who was brave

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43 The excellent recent collection of essays edited by S. Doran and T.S. Freeman, *Tudors and Stuarts on film. Historical perspectives* (Basingstoke, 2009), contains several important reflections on an endlessly fascinating subject: for example, S. Doran, 'From Hatfield to Hollywood: Elizabeth I on film', pp. 88–105; V. Westbrook, '"Elizabeth: the golden age": a sign of the times?', pp. 164–77; and P.E.J. Hammer, '"The private lives of Elizabeth and Essex" and the romanticization of Elizabethan politics', pp. 190–203.

44 See A.D. Boyer, 'The trial of Sir Walter Raleigh: the law of treason, the trial of treason and the origins of the confrontation clause', *Mississippi law journal*, 74 (2005), 869–901.

enough to answer back. Furthermore, the indirect consequences of 1603 include Ralegh's literary monument, his massive, unfinished *History of the world.*

The *History* was central to the preservation of Ralegh's memory in the seventeenth century. Nevertheless, he is not widely remembered today for his skills as a writer. His book remained influential for many years, but everything has its day, and his *History*, like other histories, eventually went out of fashion. It sat with other respected yet unread volumes on the folio shelves of gentlemen's libraries, gathering dust. John Milton was impressed; a century later, Samuel Johnson and David Hume respected the achievement but were less enthusiastic. Ralegh's limitations as a scholar of the ancient world, apparent even to his contemporaries, were steadily exposed, and today all the chronologies and painstaking analyses of biblical inconsistencies only frustrate the reader seeking to engage with this great work.

This leaves us with the Last Act, and the power of a simple story. Events in 1618, for all their twists and turns, are both human and straightforward. An old man loses his son in a desperate venture after gold and then comes home to face the consequences of his actions. Confronted by the anger of his king, he is put to death without a fresh trial for offences committed long before. Such simplicity suits a process of memorialization. The events of 1618, of course, differs from those of 1603 in the final drama: the scaffold and the death of Ralegh. Death confers martyrdom; death makes the commanding impression. This sense of 'nobility in extremis' is an undercurrent to so much that follows. In the early 1700s Ralegh became a soldier, a stock military hero. Military heroes face death with fortitude, just as Ralegh had done. The very popular 1719 *Tragedy of Sir Walter Raleigh* by George Sewell portrayed him once again as a victim of underhand Spanish scheming, but also helped break new ground by fashioning him as an honest, virtuous family man. The ironies and tangles in his love life were forgotten; plays dwelling on human values and lives in jeopardy will always pack a theatre.

The scaffold draws out all the complexities of his career. How was it that a man blessed with so many gifts and talents could come to this? Did the flaws in his character outweigh the gifts? Is genius perhaps prone to self-destruct? These were questions asked by the young Edward Gibbon, a would-be biographer searching for a subject during the 1760s. For a time Ralegh was his choice, and indeed his hero. But Gibbon abandoned the project when he realized that other writers had already studied Ralegh's career, and that biography, if pursued, must lead eventually to neglect and oblivion. Like Ralegh, this was a prospect that Gibbon could not easily contemplate.

American revolutionaries familiar with Ralegh's role in the prehistory of east coast colonization pointed to his 'martyrdom' for a cause, appreciating his value as an icon of opposition to royal tyranny. In 1775, after the Battle of Bunker Hill, the *Pennsylvania Packet* newspaper summoned in tribute to Dr Joseph Warren,

who died on the battlefield, a parade of sacrificed past patriots led by Ralegh.[45] Revolutionary America named a frigate after him too. The US Navy has a habit of naming its warships for cities across the 50 states, but this *Raleigh* commemorated the man. It was only a decade or so later that the carefully planned capital of North Carolina took the name of the state's most famous founding father and preserved the association thereafter in all sorts of ways. If you seek his memorial, look at the catalogue of streets and place names in this city.[46] As H.G. Jones has shown, the legacy is not limited to North Carolina. Ralegh has given his name to towns and counties in seven states, to trains, shops and car races as well as hotels and, predictably, several brands of tobacco.[47]

In the nineteenth century, Ralegh was remembered for new, equally topical reasons. Never forgotten by American republicans, he now became an imperial founding hero on both sides of the Atlantic, as an empire grew and the west was won. Assisted by a particular kind of hindsight, the expeditions to Roanoke fuelled the potent symbolism of Ralegh as a pioneer of divinely ordained expansion. The poet Joel Barlow in his epic 'Columbiad' conjured a general set on further conquests, 'his eye, bent forward, ardent and sublime,/Seem'd piercing nature and evolving time;/Beside him stood a globe, whose figure traced/A future empire in each present waste'. One hundred years later, John Buchan, in a biography of Ralegh written for children, argued that 'the British Empire of to-day, and the Republic of the United States, are alike built on dreams'.[48] For those living with the reality of great empires, the eloquence of Ralegh's letters and works took on a prophetic, visionary quality, all the more potent when coming from a man put to death for pursuing so ambitiously a grand imperial design.

But the design has had to be purified of any lust for silver and gold. Imperial pioneers should be motivated by good intentions. Victorian popular histories, and even the scholarly Edward Edwards in his painstaking *Life and letters*, simplified Ralegh into an uncorrupted lad with his eyes fixed on distant horizons, as in Millais's 'Boyhood of Raleigh', or the gallant adventurer setting sail from Plymouth towards the sunset. No matter that he was always sick while at sea and had something of a reputation as a 'Jonah' at court![49] Of course, a flaw lurks within every hero. Always there is a suggestion that the story will end unhappily. Ralegh had his flaws, knew that he had them and could articulate his own first-hand experience of the downbeat ending. As everyone knows, the *History of the world* draws to a close in a hymn to death: all-conquering death. Princes, statesmen, men

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45 R. Lawson-Peebles, 'The many faces of Sir Walter Raleigh', *History today*, 48 (1998), 17–24 (21).

46 H.G. Jones, 'The Americanization of Raleigh', in J. Youings (ed.), *Raleigh in Exeter, 1985. Privateering and colonisation in the reign of Elizabeth I* (Exeter, 1985), pp. 73–89 (pp. 77–8).

47 Ibid., pp. 78–9.

48 Lawson-Peebles, 'The many faces of Sir Walter Raleigh', 23–4.

49 R.B. Wernham, *The return of the armadas* (Oxford, 1994), p. 270.

and women might strut about for a time, but in the end every one of them is shut up in death, and only death prevails. In a supposedly Christian book, God and a sense of redemption are signally absent from this peroration. Death is the fate of knights errant; death is the destiny of the hero. Ralegh wrote his own epitaph and, in 1618, he fulfilled his destiny.

Of course, 1618 or no 1618, it is possible to argue that dusty oblivion is now at last closing in on Ralegh. With Francis Bacon he remains the quintessential Renaissance man. His early biographer John Shirley made the point in a way well suited to his seventeenth-century readership. Ralegh had been 'statesman, seaman, souldier, chymist, or chronologer, for in all these he did excel. He could make every thing he read or heard his own, and his own he could easily improve to the greatest advantage'.[50] Much more recently, Andrew Marr, a Scot who went in search of the English for a BBC Radio 4 series, came away astonished by the complexity of Ralegh's life. Today, however, the specialist prevails over the polymath. Walter is no longer a name for a hero, and this particular Walter suffers from the now poisonous associations with tobacco and with Ireland. The Beatles lyric – 'Although I'm so tired I'll have another cigarette/And curse Sir Walter Raleigh, He was such a stupid get' – and Seamus Heaney's expansion of the swisser-swatter seducer into the ravisher of a nation in the 'Ocean's Love to Ireland' both reject virtue in the energetic, misguided, womanizing addict.[51] Any purpose in remembering a flamboyant figure from the Elizabethan court fades away, mashed into legends surrounding the introduction of potatoes, blurred in a puff of smoke.

A purpose, however, is not always necessary. Still the name means *something* to many people; it endures. All heroes live on in the unconscious gesture. Men still spread their coats and cloaks for women they admire; in 2007 alone, such gallantry surfaced in subway advertisements for a dating service and in the Labour Party's deputy leadership campaign.[52] There always seems to be scope for reinvention. In John Madden's 1998 film *Shakespeare in Love*, Queen Elizabeth (played by Judi Dench) eyes the puddle, glares at a group of dawdling courtiers and, in a very post-modern way, wonders where Ralegh is when you need him. Collective memory, as we know, is constantly under reconstruction.[53]

Acknowledging complexity gets us somewhere close to the truth of the matter. Ability and fragility, apt eloquence and crass misjudgement, sympathy and disregard, energy and despair – the contrasts and extremes sum Ralegh up. Utter confidence masks doubt. Doubt manifests itself in sarcasm. Sarcasm sometimes

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[50] J. Shirley, *The life of the valiant & learned Sir Walter Rawleigh* (London, 1677), p. 242.

[51] The Beatles, 'I'm so tired', from the *White Album* (1968); S. Heaney, *North* (London, 1975).

[52] The dating agency was Lavalife, while Stephen Pound played Ralegh to Hazel Blears's Gloriana!

[53] M. Halbwachs, *On collective memory*, trans. L.A. Coser (Chicago, 1992).

topples into pessimism and self-pity, verging in turn on the Churchillian 'black dog'. As Sir Robert Naunton observed, Ralegh was 'fortune's tennis-ball'. Queen Elizabeth had 'tossed him up of nothing, and to and fro to greatness, and from thence down to little more than to what therein she found him, a bare gentleman'.[54] The stability in his life came from without. He found little peace within.

The bleak drama of 1618 takes us as close as we can ever get to the man himself. These events demonstrate the courage, the eloquence and also the abiding dark heart, the cynicism cloaked in eloquence and theatre which checks the admiration. There is something chilling in the picture of Ralegh on the scaffold, talking on and on, stirring the religious fervour of a crowd, pacing to and fro, all the while rejecting any reconciliation with those who have brought him to that place. There is also something reassuring, human, if ultimately depressing, in the attempts of educated, principled men to stop what they clearly see to be, if not a miscarriage, then an extremity of justice, unwisely applied. Without this fashioning of his death, and without the enduring scholarly and public response to that fashioning, Ralegh flits transiently, irrelevantly, through the history of his country. Dying in this manner and in these circumstances he leaves a formidable legacy, not just for the seventeenth century but for centuries beyond.

[54] Naunton, *Fragmenta regalia*, p. 30.

Chapter 9

Thomas Harriot and the Mariner's Culture: On Board a Transatlantic Ship in 1585

Pascal Brioist

> Heigh, my hearts! Cheerly, cheerly, my hearts!
> yare, yare! Take in the topsail. Tend to the
> master's whistle.
> Blow, till thou burst thy wind,
> if room enough!
> …
> Down with the topmast! yare! lower, lower! Bring
> her to try with main-course.
> …
> Lay her a-hold, a hold! Set her two courses! Off to sea again: Lay her off![1]

These words, shouted in despair at sailors by the character of the boatswain in Shakespeare's *The Tempest*, refer to a ship initially bound for Virginia. It is acknowledged that the bard's play was written a few years after the real shipwreck of a Virginia company's merchantman.[2] It is also acknowledged that Shakespeare himself knew mariners who possibly, served as technicians on stage, handling the ropes and the pulleys of theatrical machinery. He certainly had the opportunity of hearing them tell stories of seafaring and of using their highly specialized vocabulary. About 25 years earlier, Thomas Harriot had a better opportunity of recording the language of sailing men, on board the ship on which he crossed the ocean to land in America.

Harriot had some experience of seamanship and of seafaring. After having graduated in Oxford in 1580 and having been trained there in astronomy, he developed an interest in questions of navigation and started to gather information from men who had crossed the ocean.[3] In around 1583, in Sir Walter Ralegh's

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1 W. Shakespeare, *The Tempest*, Act I, Sc. 1, in the Clarendon Press edition of *The complete works* (Oxford, 1989).

2 For an astonishing account of that episode that took place in 1609, see M. Rediker, *The many-headed hydra. Sailors, slaves, commoners and the hidden history of the revolutionary Atlantic* (Boston, 2000). See esp. Chapter 1, 'The wreck of the sea-venture'.

3 See D.B. Quinn, 'Thomas Harriot and the new world', in J.W. Shirley (ed.), *Thomas Harriot. Renaissance scientist* (Oxford, 1974), pp. 36ff.

Durham House, London, he gave private instruction in the use of instruments and charts to his patron and to his expedition captains. He even wrote a textbook, entitled the 'Arcticon', to initiate them in cosmography.[4] Ralegh's dream of taking on Gilbert's task of exploring North America took shape in that context.

We know for sure that in 1585 Harriot took part in the expedition to Virginia commanded by Sir Richard Grenville. He was certainly then on board the small fleet's flagship together with the admiral, who was Ralegh's relative, and with his associate John White.[5] This hypothesis is strengthened by the fact that at the end of March, the *Tyger* left London, where Harriot lived, to join the rest of the fleet in Plymouth.

It has also been suggested that Harriot had been previously a member of the 1584 expedition launched by Ralegh led by Philip Amadas and Arthur Barlow. This would count for another transatlantic voyage.[6] Furthermore, after 1585, Harriot encountered many other opportunities to sail since he travelled many times to Ireland, where Ralegh and he, on a more modest scale, were endowed with colonial estates in the 1590s.[7] It is clear, then, that in his mid-twenties, he had gained a familiarity with the ocean. His notes on the officers of a ship and on rigging, though written around 1608, are, with a certain degree of probability, a reminiscence of these past experiences.[8] These are fascinating pieces of evidence

4 See BL Add. MS 6788, f. 487: 'By truth of demonstration which I have uttered in my *Arcticon*, which here for brevity sake I omit, I have probed that the … being above the levell of the water…'. Unfortunately, the 'Arcticon' has not survived. It was probably much later, while serving Henry Percy, that Harriot wrote his 'doctrine of nautical triangles'.

5 Evidence for John White's and Harriot's presence on the *Tyger* is in *Hakluyt's principal voyages* (1589), where the two men are mentioned as part of the team sent to Wococon on a reconnaissance mission: 'The eleventh day, the general accompanied in his tilt boat with Master John Arundell, Master Stukeley, and divers other gentlemen, Master Lane, Master Cavendish, Master Hariot, and twenty other in the new pinnace; Captain Amadas, Captain Clarke, with ten others in a shipboat,; Francis Brooke and John White un another shipboat.' It seems that the new pinnace and the other two shipboats were accompanying the *Tyger*.

6 On this issue, the only evidence we have is the allusion of the artist John White, Harriot's future collaborator, about his own participation to the voyage of 1584. Considering the strength of Harriot and White's association, it is indeed doubtful that the former stayed in England if the latter went away. See Quinn, 'Thomas Harriot and the New World', 38.

7 Ralegh had received 40,000 acres from the queen, in the area of Youghal, in the province of Munster. Harriot was himself endowed with Molana Abbey, north of Youghal, and White had a house at Newtone in Kilmore. Harriot and White drew maps of these lands and curiously, on one of them, the cartographer uses the phonetic script he invented to record the Algonquian language (cf. map F1033, P/49(29), National Maritime Museum).

8 One can find them in BL Add. MS 6788, ff. 21–48, next to papers on shipbuilding, fortification and military tactics. The leaves are not in order, but numbers in front of diverse entries allow a reordering, as if the author had planned to write a treatise. For the attribution of a date to these pages, see J.V. Pepper, 'Harriot's manuscript on shipbuilding and rigging

about sixteenth-century popular culture. The way Harriot records the very words he heard on board opens a door to a past that usually remains silent. Here, sea culture is not understood from the top, as it is generally presented by sixteenth-century authors such as Edward Wright, who denounced 'certaine errors in navigation' committed by sailors, but rather from the deck, with those who handled the ropes and worked the capstans.[9] This is exceptional because when Harriot wrote, for instance, 'To bouse is hale. Come shoo bouse here', he registered the very voice of the men he heard.

Therefore I propose, borrowing from Stephen Greenblatt's seminal expression, a little 'shamanism' exercise. After a quick survey of the ships involved in the 1584 and 1585 voyages, I will focus on Harriot's way of observing the activity of men of board, then I will try to make sense of Harriot's description of an Elizabethan rigging and finally I will turn to his account of the manning of a great wooden ship.

The ships of the 1584 and 1585 voyages

The vessels used for the colonization voyages were not necessarily purpose-built. Some were small ships such as pinnaces, sometimes even smaller than English coasters; others were, on the contrary, defensible merchantmen carrying artillery. In 1584 Philip Amadas and Arthur Barlow sailed with two exploration vessels that Barlow qualified as 'barks'. On the voyage of 1585, the fleet was composed of seven boats: the *Tyger* and the *Roebuck*, each of about 140 tons, the *Lion*, of 100 tons, the *Elizabeth*, of 50 tons and the *Dorothy*, a small bark and a pinnace of about 30 tons. Pinnaces, generally clinker-built, ranged between 28 and 32 feet in length, 6 and 7 feet in breadth and were driven by sail and by oars (10 or so). They served to transport men and gear to the shore, to establish contacts between larger ships or for diverse reconnaissance missions.

Vessels like Amadas's barks, the *Tyger*, the *Elizabeth* or the *Roebuck* were among the 15 per cent of the largest ships of the realm.[10] They were carvel-built,

(c. 1608–1610)', in D. Howse (ed.), *Five hundred years of nautical science 1400–1900. Proceedings of the third international reunion for the history of nautical science and hydrography* (Greenwich, 1981), pp. 204–16. Pepper's argument is internal evidence: the reference of the pages on shipbuilding to the *Prince Royal*, launched in 1610, and Harriot's claim that he invented a method of determining the best dimension of masts in February 1608. Since the *ductus* of Harriot's handwriting is the same on these pages and on the pages concerning rigging, we have to agree with Pepper.

[9] See E. Wright, *Certaine errors in navigation* (London, 1599). There is an extensive literature of this kind, dealing with the handling of navigation instruments. One could quote, for instance, William Bourne's *A regiment for the sea* (London, 1576), John Davis's *The seaman's secret* (1595) or John Tapp's *The art of navigation* (1599).

[10] Cf. the 1577 government survey of merchant shipping, quoted by M.M. Oppenheim in *A history of the administration of the Royal Navy. 1509–1660* (London, 1896), pp. 173–4. The *Roebuck* was rated a flyboat of 140 tons and the *Lion* another flyboat of 100 tons. The

a technique fully mastered by English shipwrights of the day. Harriot gves us some clues for imagining the dimensions of such galleons when he notices, in accordance with the builder Mathew Baker's rule, that 'a ship whose depth 10 foote, width 20, length 50 by the keele is of burden 100 tons' (f. 41). We are also informed that the *Tyger*, rebuilt in 1570 in the Thames shipyards from a previous model, was 149 tons of burden. Her keel was 50 feet long, her beam 23 feet and her depth-in-hold 13 feet.[11] This last dimension means the ship had a much bigger hold

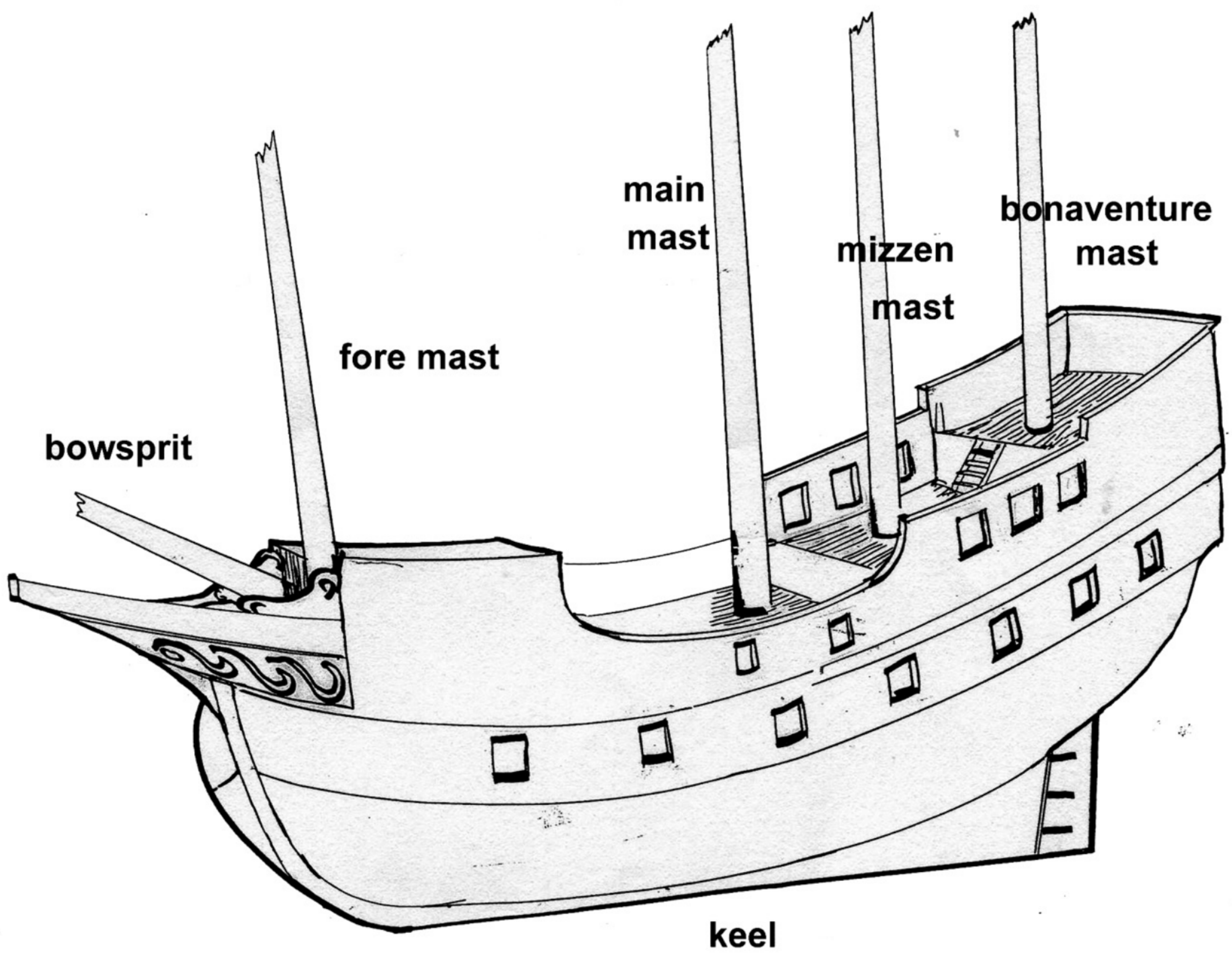

Figure 9.1 Hull and masts of a ship of the type used for the voyages to Virginia

Dorothy and the *Elizabeth* were small barks or large pinnaces of 50 tons. The expedition also used smaller pinnaces of 20 to 30 tons like the one lost by the *Tyger* during a storm in the 'bay of Portingal', a 'double oared whery of four oares belonginge to Sir Walter Ralegh', a tilt-boat, a light horseman (that Ralph Lane took to go up the Roanoke River) and a shallop.

[11] There is some debate about the *Tyger* being an upgraded version of Henry VIII's galeass. Some authors claim that the discrepancies between the vessel that can be seen on the Anthony Roll and the one shown on John White's drawings could account for a trade between the old version of the *Tyger* against Sir William Winter's *Seadragon*. Cf. J.L. Humber, *Backgrounds and preparations for the Roanoke voyages. 1584–1590* (Chapel Hill, NC, 1986), pp. 31–3.

than usual and was fit for long-distance travel.[12] She could provide 9 tons of cargo space per person. Each of the big vessels could carry between 50 and 160 sailors and passengers, but they were certainly more crowded than the Mayflower 30 years later. We also learn from the Spanish ambassador Bernardino de Mendoza, in a ciphered letter he sent to King Philip II evaluating the strength of Ralegh's fleet as one of 600 men, that the *Tyger* had 'five guns on each side … and two demi-culverins in the bows'.[13] From Grenville's mention of a distance measured as 'a falcon shot', we may deduce that the boat might have carried small firearms.

The vessels used in Barlow and Grenville's expeditions were three-masted: they had a mainmast, a foremast, a mizzenmast and a bowsprit. The squared canvas sails (rigging the main mast, the foremast and the bowsprit) served for running with the wind and were very powerful tools for driving the ship rapidly. The lateen sail, a triangular canvas laced to a long yard hoisted obliquely to the mast, gave the ship manoeuvrability. It was set on the mizzenmast, close to the rudder. It served for sailing obliquely against the wind and could be used in a variety of wind conditions. Unfortunately, the lateen sail suffered from a number of drawbacks: it required a larger crew, it was altogether less powerful in propelling the ship, it could not be divided into units that would have been convenient for handling (this was possible for square sails) and instead of furling sails on the yard, the yard had to be lowered. These are the reasons why in the second half of the fifteenth century and the early sixteenth century a hybrid solution to overcome these inconveniences: one that combined lateen and square-rig.

Observing men on board

Crossings were both long and trying on board the large wooden sailing ships of the day. To evaluate the duration of the trip, we can draw on the two descriptions we have of the first crossings to what would soon be named Virginia. The first voyage of 1584 was commanded by captains Philip Amadas and Arthur Barlow. They used two ships provided by Ralegh: a flagship of about 60 tons and a pinnace of about 30 tons. They left for America on 27 April and stopped in the Canaries on 10 May, before sailing on to the West Indies and arriving there on 10 June. After a second landfall, they set sail for the Cape of Florida and sighted land, what is today Carolina, on 2 July. Such a three-month trip was fairly common for a

[12] For those figures, see M. Daeffler, 'Formes de carène et navires de combat', *Histoire maritime*, 1 (2004), 67. On the *Tyger*'s important supplies, see D.B. Quinn, *The Roanoke voyages, 1584–1590*, 2 vols. (London, 1955); and G. Milton, *Big chief Elizabeth. How England's adventurers gambled and won the New World* (London, 2000), pp. 83–6.

[13] Quoted in Milton, *Big chief Elizabeth*, p. 93. For the Mendoza letter, see K. Reding. 'Letter of Gonzalo Mendez de Canzo to Philip II', *Georgia historical quarterly*, 8 (1924), 215–28.

crossing, although Barlow admits he wasted time on its way to Havana, 'keeping a more south-easterly course than was needful'.

The second colonization voyage took place at about the same time. Indeed, when Sir Richard Grenville began his transatlantic crossing to Virginia on 9 April 1585 with his small fleet, he reached the Canaries from Plymouth within only five days; from there, he reached Domenica within seven days and St John a few days later. The company stayed about one month on that island and spent time building a new pinnace (the fleet having been scattered by a storm near the Canaries). The *Tyger* and the reduced fleet then left for Hispaniola and anchored there on 1 June. The fleet coasted for a while between various landings – Caicos, Guanima, Cyguateo and Florida – and arrived finally at Wocokon on 26 June and at Wingina on 3 July. The trip, as we reckon, took three months altogether, a delay caused by numerous encounters with the Spanish and the need for stops for resupplying and repairs.

Thus, on these different occasions (1584, 1585 and even later), Harriot had plenty of time to observe his fellow sailors. The question of how he saw them is puzzling. One should certainly consider the fact that his text is full of expressions like 'they do this' (for instance, 'they use it but in fair weather': f.29), 'they say that' ('a great peace of tymber … which they call a davit': f. 29) as if they belonged to another humanity. In an ordered society like the Elizabethan one, this is hardly surprising, since Harriot certainly felt he was in some way part of the society of gentlemen, as Ralegh's private teacher. But he also watched the mariners with sympathy, such as when he wrote (f. 32): 'The breming of a ship … to put on stuff. This is not well payed. Pay on more.'

Harriot's consciousness of hierarchy and order is nevertheless exemplified by his 'Notes of the officers of a ship' (f. 21). On this page we are given the shares attributed to each officer on a man of war: 8 for a captayne, 7 for the master, 6 for the two masters mates, 4 for the four quarter-masters, 4 for the boatswain, 3 for the boatswain's mate, 5 for the two midshipmen, 2 for the surgeon, 2 and 1½ for the swabber and his mate, 5 or 4 for the Master gunner, 4 or 3 for his mate, 2½ for the quarter gunners, 2 for the gunners, 5 or 4 for the purser, 3 for the steward, 2 for the cooper, and 4 and 3 for the cook and his mate. The 'shifters of victuayls', the soldiers, the drummers and the trumpeters got only 1 share. A lieutenant would receive 6 shares and a corporal 3. On board a merchant ship, Harriot noticed that there are marshals (which are for him the equivalent of captains in the navy).[14]

Next to the role of officers are considerations on watches that provide us with hints on the organization of time and work on board. There were altogether 6 watches of 4 hours each: from 12 to 4 o'clock (the midnight watch), from 4 to 8 and so on. Each watch corresponded to the flow of 8 sand glasses and was terminated by the ringing of a watch bell hanging from the castle of the ship (one of these has been recovered from the site of the *Mary Rose*).[15] Harriot's

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[14] This hierarchy can be compared to that of Thomas Smith in his *Sea grammar* (London, 1627), p. 72.

[15] Cf. C.S. Knighton and D. Loades, *Letters from the Mary Rose* (London, 2002), p. 72.

observation that 'the watch from 4 to 8 at night is divided into two partes' proves that his contemporaries were aware of the dangers of night-time navigation. A little diagram also shows that watches, for each board side, were divided between two teams: the master's side and the mate's side (a and b), a complex setting that ascertained fair treatment for all.

The language of the mariners seems to be, for Harriot, a source of wonder, and he clearly took pleasure in noting down expressions such as 'hale fythe end' (f. 29) or 'come shoo bouse here' (f. 22). The question of the western accent of the sailors, who were often recruited in Devon or Dorset, and of its comprehension by Harriot, is worth asking, since in some cases Harriot is not even sure of what he heard. On f. 27, for instance, he wrote '(vingles I thinke) that go round about the aylet holes'. Of course, it would have been inappropriate for a scholar to go to the sailors and make them repeat themselves, especially while they were performing their duties. This sentence written down by Harriot also questions the conditions in which he initially took notes (unless it questions the reliability of his memory when he finally assembled his small treatise).

Harriot's description of an Elizabethan rigging

In the sixteenth century, a transatlantic sailing ship was one of the most wondrous machines a man could see in his lifetime. It was an impressive assemblage of sails, yards, pulleys, blocks and ropes whose purpose was to enslave windpower with a minimum workforce, transforming a natural motion into an artificial one. From the days of the pseudo-Aristotle, Western scholars had known that the principle of driving a boat was essentially one of leverage, the rudder being the lever and the wind captured by the sail being the driving force.[16] Over the centuries, however, ships had become steadily more complex, combining square sails and lateen sails, standing and running rigging. The bigger the ship or vessel was, the larger was the spread of canvas required for moving it, and consequently, the more elaborate was the rigging, adding topmasts and topsails, and even topgallants above them.

As a result, a sailor had to remember the names of a multiplicity of masts, yards, sails, ropes and pulleys (around 80 ropes, and 150 pulleys and blocks, on board a big vessel).[17] This was understood as a question of survival. Confusing two cables when the sea was rough, or not reacting quickly enough, could lead to disaster, as nearly happened to the *Tyger* in a storm near Pamlico Sound on 7 June 1585. Probably, each man had an assigned task starboard side or port side, but

[16] Cf. Aristotle, 'Mechanical problems', 3–5, in *Aristotle. Minor works*, trans. W.S. Hett [Loeb Classical Library, 307] (Cambridge, MA and London, 1993), pp. 353–61.

[17] On this issue for a later period, see M. Rediker, *Between the devil and the deep blue sea* (Cambridge, 1989).

since the crew could change from one trip to another, it was essential for each man to know more or less the entire vocabulary.[18]

To emulate the sailors, Harriot proceeded as a lexicographer in exactly the same way as he collected information about the language of the Algonquian Indians. He thus methodically reduced the mechanical complexity of a sailing ship, and his progress seems quite logical once the pages are reordered as he meant them to be. He distinguished ropes for masts, ropes for yards, ropes for sails and ropes for anchors. For the five masts (bolesprit, mayne mast, fore mast, bonaventure mizzen and after mizzen), he considered five entries: '1.Forestay, 2.Backstay standing or running, 3.shroudes, 4.top ropes and 5.tackles.'[19] The stays were strong ropes employed to support the mast; one can distinguish between fore-stays and back-stays. Harriot explained how they should be set:

> Fore stay of the fore topmast item; from the top of the foretopmast to the hier parte of the boltsprit. Fore stay of the foremast from the top of the foremast to the boltsprit. Backstayes at the fore topmast from the top yards to the aftermost parte of the forecastle, 2 or 3 … All the topmasts have a forestay and a backstay. Backstays are standing backstays and running backstays. Two standing backstays are either side and belong to one topmaste ward to the outside of the ship; and the other within being also two.[20]

This network of ropes secured the masts to the hull of the ship. They held the masts up against the wind. Harriot said nothing about the tension created by the stays between the masts, but it is quite clear that the stress caused by a violent wind could be tuned by loosening the rigging in some areas, thanks to collars and 'deadman-eyes' that fixed the stays to the masts.[21] The 'shroudes' fixed down the lower part of the masts from side to side, starboard and port, and met lateral pressure, somewhat like the backstays. Their ratlines (or ratlings) were used by

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18 BL Add. MS 6788, f. 21. Ralegh realized later on that using impressed amateur seamen was counter-productive since they 'were so ignorant in sea-service as that as they know not the name of a rope and [are] therefore insufficient for such labor'. Quoted in Milton, *Big chief Elizabeth*, p. 97.

19 The mention of both a bonaventure mizzen and an after mizzen in BL Add. MS 6788, f. 28, might prove that Harriot was on board the *Tyger*, since this ship possessed these two masts.

20 BL Add. MS 6788, f. 28.

21 See BL Add. MS 6788, f. 22: '12. Collars of the stayes are such ropes coming about the mastes (or timber) to the ende being applied & hoyst about a dedmaneye, & the lower end of the stay hath a deadmaneye also, through which eyes is reeved a rope to and fro to hale the staye & coller tought together & this rope is called a lannier.' A little sketch in the manuscript clarifies this description.

sailors as steps to climb up to the crosse-trees.[22] Harriot mentions elsewhere (quite inappropriately) the 'catharpins', small ropes running in little blocks, fastened to the shroudes, from one side of the ship to the other. Their function was to 'to keepe the shroudes tought'. These standing ropes were completed by running ropes or tackle, fulfilling a variety of purposes. In his notes, he took care in detailing the different parts of tackle. A very useful sketch explains perfectly how, by using them, things could be easily heaved in and out of the ship thanks to a combination of blocks with shivers and pulleys.

Having dealt with ropes for masts, Harriot turned to the case of ropes for yards which were all sorts of running rigging: halyards, slings, brakes, lifts, puddings and loops. Halyards were usually employed to hoist or lower the yards. Ties, fastened to the head of the mast, supported the yards at the cross tree. Slings were used to fasten the yard when it was up. Brakes, at the end of the yards, were useful for orienting the yards port or starboard.[23] Lifts, or topping lifts as they are sometimes called, were complementary to the halyards. Fastened at the end of the yards and their other extremity joining the topmast, they served to hold up the yards so that the weight of the boom would not affect the set of the sail.[24] Using pulleys, they also served to lift the yards. Puddings kept the sails from slipping or sliding, and loopes were tackle fastened at the yards with a block to haul things up. As a scrupulous scholar, Harriot also mentioned ropes (brest ropes) which encircled the masts and kept the yard close to it.[25]

Ropes for yards had in reality three main functions: to hale them up, to orientate them or to hold them strongly. Sails were a bit more complicated. For them, Harriot distinguished eight sorts of cordage haled with the help of blocks and pulleys: bolts, vingles, bowlines, sheates and tackles, clew garnets, martinet lines and luft hookes.

Bolts were those ropes sown about every sail for the better handling of the sail. Bowlines were attached to the weather leech of a square sail (the vertical edge of it) to hold the leech forward when sailing close-hauled.[26] The clew garnets were

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[22] 'The mastes themselves have no backstays but have shroudes that come from the crosse trees under the tops to the clayne wales': BL Add. MS 6788, f. 28.

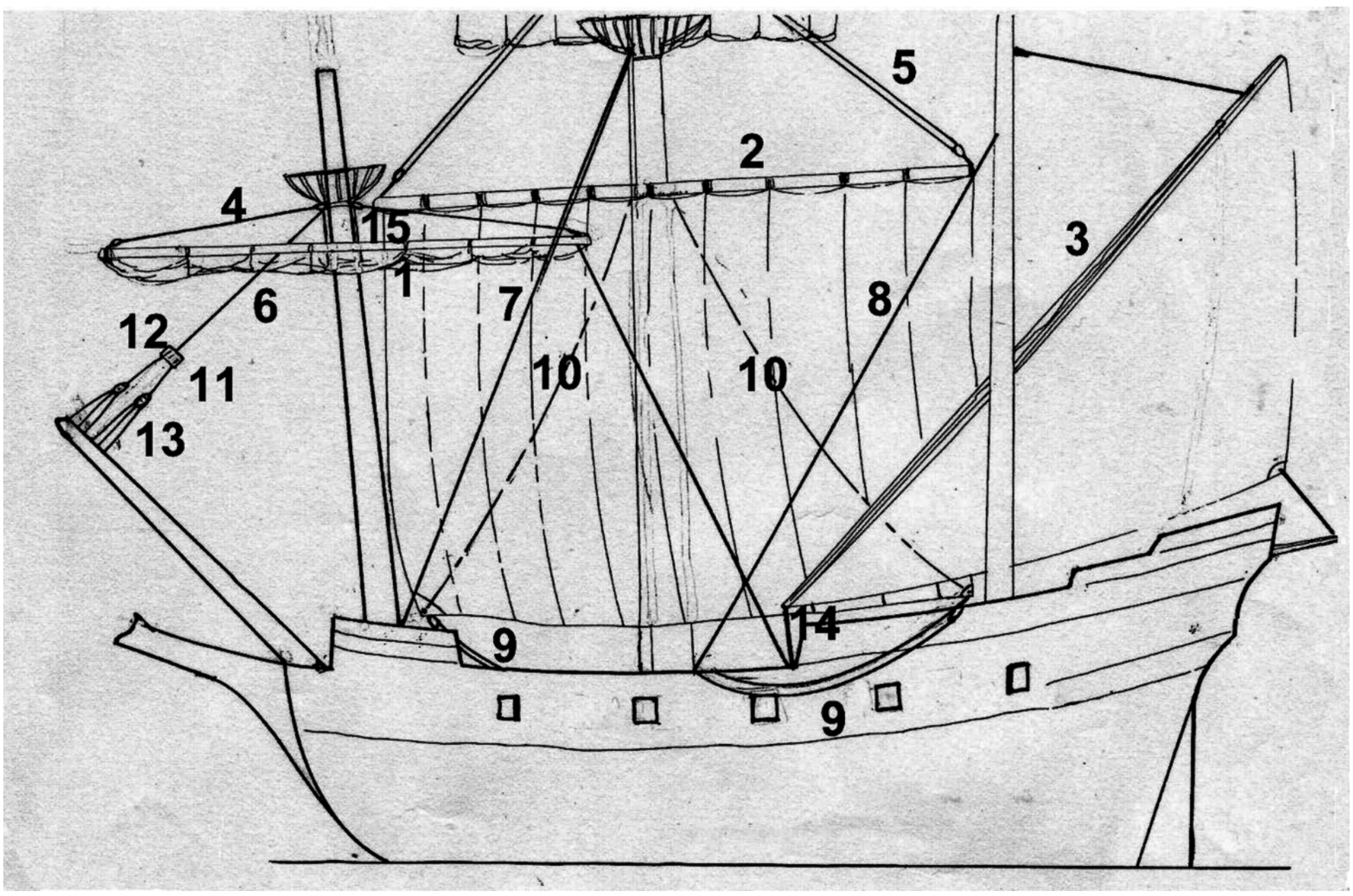
[23] On BL Add. MS 6788, f. 27, Harriot specifically stated that they did not exist for the mizzen mast: '4. Brakes, for all the yardes, except mizen yardes. Fastened at the endes of the yardes & coming thorough pullyes to turne about the yards.'

[24] Cf. BL Add. MS 6788, f. 27: '5. Liftes for all yardes. Fastened at the endes of the yardes and coming thorough pullyes aloft; to lift the ende of the yardes up.'

[25] Cf. BL Add. MS 6788, f. 27: '7. Puddinges: short peeces of ropes nayled about the yardes about a foot or 1 ½ asunder to keep the Raubins (of the sayles) from slipping or sliding. 8. The loopes have a pendant part fastened at the yards with a block (as the tackles) & a running part. One end fast, & the other end to hale up. It reeved through the block of the pendant.'

[26] Cf. BL Add. MS 6788, f. 24: 'Bowlines: His office is to hale downe the top sayle (the halyardes being let go). & to hale up the clewes to the top sayle and arme.'

made fast to the clew (the inferior part of the main sail) and run in a block to the middle of the yard. Their function was to hoist the clew of the sail to the middle of the yard, while the purpose of the tacks (spelled ‘tackles’ by Harriot) was to hale the clews downwards and forwards. The function of the sheets (spelled ‘sheates’ by Harriot), on the other hand, was to hale the clews downward and aft. Clew garnets, tacks and sheets were interconnected and allowed the ship to tack without brailing up the sail. Martinets were those lines attached to the legs of the leech of a sail, reeved at the top mast head; they were used to close up the sail.[27] Thanks to

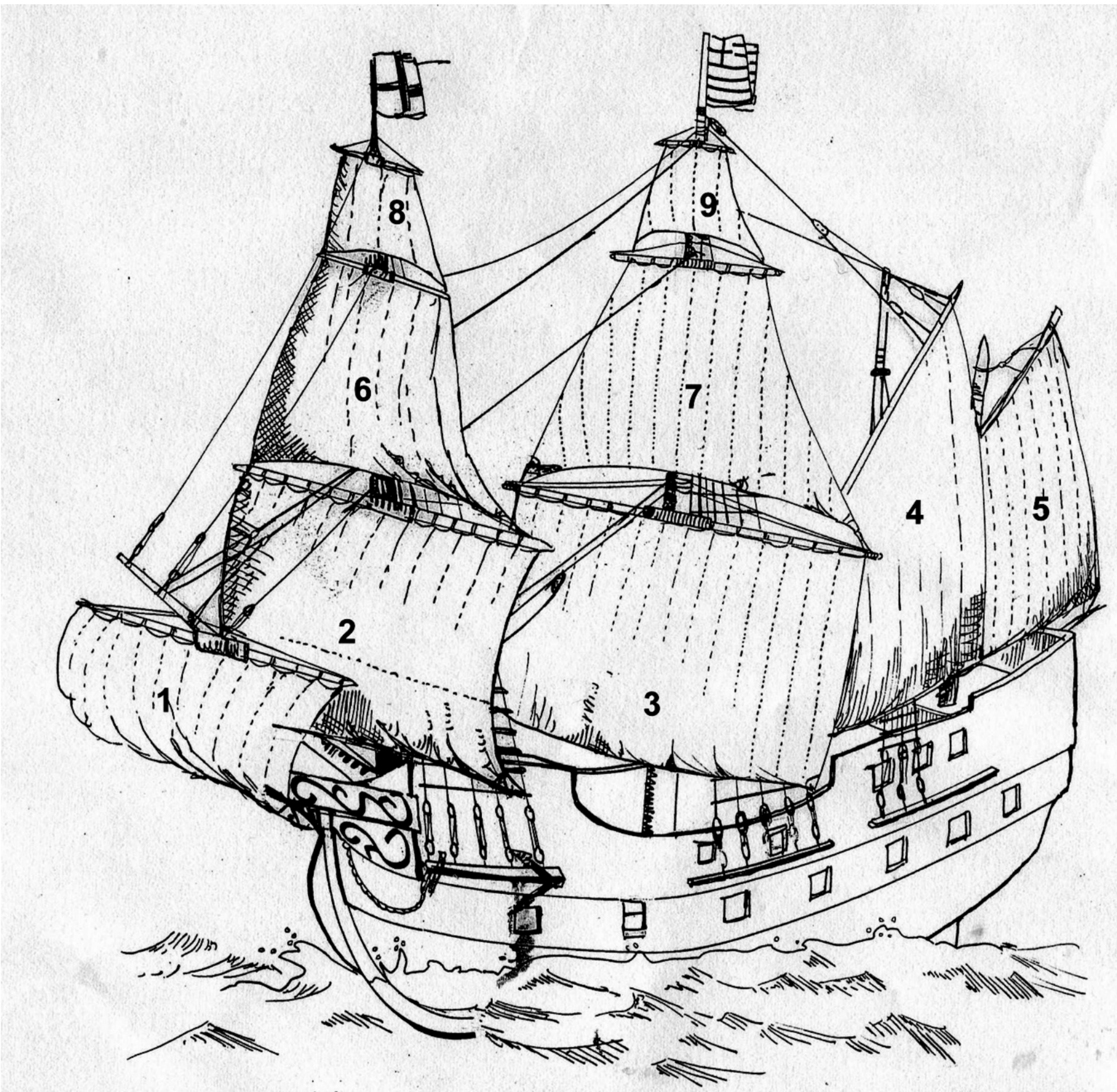


1. foreyard
2. main yard
3. mizzen yard
4. lift
5. top lift
6. fore stay
7. main stay
8. mizzen stay
9. sheet
10. clew garnet
11. martinet line
12. block
13. deadman-eye
14. tackle
15. halyard

Figure 9.2 Rigging of a ship of the type seen by Harriot

[27] Cf. BL Add. MS 6788, f. 25: ‘The martinet line one end is fastened under the top and reeved thorough a double pully with three shivers (two one way and one another) that is thorough the single shiver and so comes harder to a pully under the top and it goes downe by the formast shrowde there to be haled. Thorough the double shivers of the pully comes two lines one on one side and the other of the yard; at whose endes are deadmaneyes, fast into the bote rope on the side and maken crowfeet. The use is by holding the martinet line. In like sort also are the martinet at the foresayle.’

Harriot, we learn that very early on, the deadman-eyes were set one forward and one aft and that they were connected to a double shiver.[28]



1. spritsail
2. foresail
3. main sail
4. mizzen sail
5. bonaventure sail
6. fore topsail
7. main top sail
8. foresail top gallant
9. main top gallant
10. sheate anchor

Figure 9.3 Sails of a ship of the type seen by Harriot

[28] See Pepper, 'Harriot's manuscript on shipbuilding and rigging', p 214, correcting R.G. Anderson, *Seventeenth century rigging* (London, 1955) on the setting of the deadeyes and their crow's feet in the early seventeenth century.

Ringles (or vingles?), according to Harriot, did go ‘round about the aylet holes like a lace to strengthen them’. They served for the lacing of the sails, like the gear he called ‘rowlings and earings’ and the ‘luft hookes’.[29] When mentioning the luft hooke, fastened at the earings of the clews, Harriot interestingly mentioned bonnets, those additional strips of canvas laced onto the foot of the sails to accelerate the speed of the ship in fair weather.[30]

Ropes for anchors were of key importance, not only to protect the ship from the current weather conditions, securing them in harbours or near the shore whatever the effects of tides and storms, but also, in the case of small anchors, to navigate under certain conditions (near a sound, up-river or downriver, etc.). They could even be used as a trick in naval combat to make the enemy think you were moving when in reality you were not. A big vessel carried various sorts of anchors: the manuscript mentions the ‘sheate anchor’, which was the greatest of them all, and smaller ones called ‘bowers’ and ‘kedgers’. There was a special way of using the ‘kedgers’ to counteract the effect of the tide or the effect of the stream by moving or turning the ship when navigating (kedging) up-river or downriver, which Harriot explained convincingly:

> There is a smale Anchor with a longe cabled hawser which is called a kedging anchor to warpe upon when occasion serveth, they lay out this smale anchor with ther bait & bringinge the ende aborde they heave the shipe to it & then let drope another smale anchor under foot & then lay out the kedginge anchor agayne which they cale warpinge end. This is used to get out of a harbour when the wynd which bloweth in would serve yf you will without the harbour but they use it not but in fayre weather when the wynd doth not over blowe.[31]

One can imagine that ‘kedging’ was an important practice for ships that had to edge over the sand bar near the Wococon inlet, to play with high and low tides along the American coastline. However, they were certainly not used to sailing

[29] Cf. BL Add. MS 6788, f. 27, 3: ’Rowling made fast in the aylet holes alofte & go round about the yardes & tyed; between the puddinges which keep you from sliding or slipping. The earinges. Which are nooses at the corners of the sayle are fastened to the ende of the yard armes with such rope many times thorough’. Smith, in his *Sea grammar*, p. 21, defines earings together with the leech in the following manner: ‘The Leech of a saile is the outward side of a skirt of a saile, from the earing to the clew; and the Earing is that part of the bunt rope 24 which at all the foure corners of the saile is left open as it were a ring.’

[30] Cf. BL Add. MS 6788, f. 25: ‘The lufte hooke is a rope with a hooke having an eye or hole, thorough which a rope is reeved and spliced which use is to party the hooke into the earing of the clew of the sayle if only the bonnet be in; or bonnet is drabble be on and the other end yard, thorough a hole on the ships side and there is haled in tought and belayd to help the tacke for feare of breaking the mayne and foresayle only hand this lufte hooke.’

[31] Cf. BL Add. MS 6788, f. 29.

up the Chowan or the Roanoke River, since the sounds were too shallow to accommodate them.

Always mindful of the materiality of things, Harriot also detailed the different parts of anchors: the ring, the working end of the anchor to which the cable was attached, the eye, the hole to which the ring was fixed, the 'flooke', the spade-shaped appendages of the arm used for digging into the seabed, and the cross (now called the 'stock'), which allows the anchor to be set in such a way that the 'flooke' can become planted in the shallows. The cordage was in adapted to the size of the anchors: a stopper was a strong rope fast to the 'sheate anchor', a hawser was a smaller rope, etc.

Harriot's knowledge of rigging was that of an outsider but also that of a careful observer with a strong sense of classification. Actually, he was certainly not the only man of his time who was not a sailor to have a clear understanding of masts, yards and their ropes. This is fairly obvious if we consider a number of engravings and illustrations that we find on maps, for instance, on the Smerwick harbour map or the plan of a fort in Tallaboa Bay: painters, engravers and cartographers knew enough to represent vessels with their main rigging ropes and equipment.[32] Furthermore, if we are to believe Humphrey Gilbert's plan for a noble academy, it was also required of gentlemen that they should become familiar with the rigging of large ships: Ralegh's relative (another one!) advocated the use of ships' models for teaching the *Arte of the shipwright*.[33] However, Harriot's manuscript presentation of rigging terminology was certainly the first elaborate classification of that kind available in England. It followed the type of arborescence scheme proposed by John Dee in his mathematical preface to Henry Billingsley's edition of *Euclid*.[34] Englishmen had to wait for another 20 years before a thesaurus of tackling and rigging was finally published. This was John Smith's *Sea grammar*.

Harriot and the description of the manning of a great wooden ship

Harriot also chose to record meticulously all the verbs of action. Some are related to ropes, such as to 'hale', to 'heave', to 'reeve', to 'ease', to 'splice', to 'furl' (the sails), to 'make fast' (to bent), to 'sling' and to 'bouse'. For instance, he wrote: 'to

[32] The Smerwick harbour map is kept at the National Archives MPF 1/75. It shows Frobisher's ship the *Aid* with all her bowlines, stays, shrouds, tacks and sheets. The plan of a fortified encampment at Mosquetal (Tallaboa Bay), Puerto Rico, reproduced in Kim Sloan, *A new world. England's first view of America* (London, 2007), p. 100, shows the fully rigged *Tyger* arriving at St John's Island, with its sails up.

[33] Cf. H. Ellis 'Copy of a plan proposed by Sir Humphrey Gilbert to Queen Elizabeth, for instituting a London Academy cir. 1570', *Archaeologia*, 21 (1844), 506–20.

[34] Cf. H. Billingsley, *The elements of geometry of the most auncient philosopher Euclide of Megara*, ed. John Day (London, 1570).

splice is to joyne two ropes or cables together upright by opening their endes'.[35] Hauling was the action of drawing up a rope, for instance, for brailing up the sail. Bousing was pulling down the sail. Reeving was drawing a rope through a block. He wrote: 'Reeve the rope is part him into his pully.'[36] All these operations depended on the musclepower of the crew. However, in order to diminish the efforts they had to make, sailors could count on pulleys and blocks: thick pieces of wood containing sheaves (called shivers by Harriot), that is, wooden or brass wheels fixed in the middle of the block with a pin.

A pulley either changes the direction of a line (which can be important for ergonomic reasons) or gives the sailor a mechanical advantage. Two or more pulleys with a rope threaded between them were called 'rope and tackle'. Their 'purchase' (that is, the mechanical advantage they would provide) depended upon the number of shivers and upon the way in which the rope was threaded. In the simplest case (for instance, when using gun-tackle), with two pulleys, the load moves half the distance of the cable so that the effort is halved. But if the distance cable is made bigger (for instance, when using a luft-tackle in which the rope turns around the two blocks), the tackle yields an advantage of three instead of two. Double pulleys were equipped with three shivers (two one way and one another) and were associated with ropes requiring great strength, for instance, the martinet lines. They would yield an advantage of four (that is, they would lift 100 lb with only 25 lb force).

The 'deadman-eyes' mentioned by Harriot function on the same principle: these small wooden disks with three holes, arranged by pairs guiding between them a line called a lanyard, pull harder on whatever they are attached to (for instance, on the shrouds).

In a few instances, Harriot also mentions mechanical winding gears: a capstan for the anchor and the 'gere' used on great ships 'to ease the ties in hurling the mayne and the fore yarde'.[37] We are not told about the aspects of these winches, probably because they were quite simple, but when the size of vessels increased later on in the seventeenth century, the technology of capstans became an important issue for scientists of both the Royal Society and the French Académie des Sciences. Under Louis XIV, the Académie offered a prize for the design of a safe and powerful capstan.

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35 BL Add. MS 6788, f. 36. This echoes Smith's definition in his *Sea grammar*, p. 22: 'Splicing is so to let one rope's end into another so they shall be as firm as if they were but one rope.'

36 BL Add. MS 6788, f. 27.

37 See BL Add. MS 6788, f. 29: 'The stopper is a stronge rope about a fathome longe one end whereof is fast to the shyppes side … a knott at one end is thrust thorough a hole in some strong peece of tymber standinge conveniently neer the foresaid place the other end to the capsten', and f. 27: 'In great ships only. The gere. To ease the ties in hurling & when it is up aloft up. For mayne and fore yarde only.'

Some other verbs identified by Harriot were related to the way sails took the wind or not, like 'Aluffe: to stay with the wind' or 'To beave up: to stay so up from the wind' or '[to furr] a ship is, when she will not leave sayle for want of breath, to make her bouder without side with timber on the plankes'.[38] A last category of verbs and orders was related to anchorage ('to move is to anker with two ankers lesse you a cuble distance'), to manning the pump ('to the water: to come to the pumpe') or to serving a cannon. Harriot was especially aware of the difficulty of handling anchor-cordage and he described with care the gestures and the tools associated with the practice (fish-hooks, davit, cats, etc.).[39]

In Harriot's papers on rigging, there are not many references to the ship's weaponry, but an allusion to gunners' tackle is interesting. A vessel like the *Tyger*, as we have seen, was equipped with 10 guns ('sakers', falcons, falconets maybe?) and two demi-culverins in the bows. They were not heavy cannons (a demi-culverin weighed 4,750 lb, a falconet 600 lb and a saker 200 lb). According to the master-gunners John and Christopher Lad, who left a treatise on the subject in 1588, they had a modest range (between 900 and 1,900feet at point blank), but the destructive effect of the shots could prove highly effective (a demi-culverin was a 9 pounder, a saker a 5 pounder and a falcon a 2½ pounder!).[40] All the guns where fitted with trunnions on the kind of four-wheeled carriages that have been found on the wreck-site of the *Mary Rose*.[41] In this context, gunners' tackles were used to roll the gun forwards and backwards as required, thanks to rings fixed to the carriage and to the side of the ship. Other ropes, secured to the hull, stopped the recoil of the gun. The role of blocks and tackles was to give a mechanical

38 BL Add. MS 6788, ff. 36, 33.

39 BL Add. MS 6788, f. 37. Cf. f. 29: 'When the anchor is upe to the hawser they have a great yrone hooke to which a rope is fast which is called the fyshooke, they lett it downe and leech it about one of the flooke of the anchore and bringe the rope out to the end of a great peace of tymber thrust out over the which they cale a davit, then cry hale fythe end. There is one either side the shype before a peece of tymber fast to the shype side standing out to a shyver at the ende, called the catte through which shyver they reeve a rope called the catte rope one ende wher of they fasten to the rynge of the Anchore & halinge this rope phe size and range utinge out the cable at the hawser they bringe it upe & then not hale out the takels they bringe the Anchor to the bord agayne…'

40 Christopher and John Lad wrote in 1586–87 a *Rule of Gunners' Art* dealing with the service of cannons at sea (cf. Bodleian Library, Rawlinson MS A. 192). It provides data on the size and range of most of the pieces. Cf. f. 21r: 'A falkenett is 6 foote longe and waightes 600 lbs. His shot in poynte blanke 900 foote–180 pases. A Mynion is 3 ynches ¼ in heyghtes and 8 fote longe and waygtes 1300 lbs His shott is 3 ynches large and reaches at poynte blancke 1350 fote or 270 yardes. A saker is 3 ynches ½ in heyghtes, it is 9 foote longe and wayghtes 200 lbs his shot is 3 ynches ¼ in heyghtes. At poynte blanke 1450 fote or 290 yardes. A demy culverynge is 4 ynches in heyghtes and 3 fote longe and wayetes 4750lbs his shot is 4 inches in heyghtes and reaches at pointe blancke 1900 fote or 360 pases.'

41 Cf. Knighton and Loades, *Letters from the Mary Rose*, pp. 112–13.

advantage to the gunner and his mates who had to run the heavy piece backwards, to load it with gunpowder, shot and wadding, to run it out and aim at the enemy before firing. The gunner William Bourne, a contemporary of Harriot, gave further recommendations:

> As for gunners that doe serve bye sea must observe this order followinge, first that they doe forese that all their great ordinance be faste breeched and that all their geare be handsome, and in a redynite and furthermore that they be verye circumspecte about their powder in the time of war and speciallye for to beware of their hynteforkes and their candells, for feare of their pondare [powder] and their fier workes and their …[?] whiche is verye dangerous and much to be feared then furthermore that you keepe your peece as maye as you can drye within and also that you keepe them fourhold cleane withoute anye kinde of drosse fallinge into them, and furthermore it is good for the gonners for to serve their peece, and for to know their perfect disperse and marke it upon the peece or else in some booke or table, and name everie peece what that it is, and where that she dothe lye in the shippes and name howe many iinches and halfe inches and qr of inches that they disperse comelye unto.[42]

For sure, Harriot was interested in the art of the gunner – at least he proved to be so later in life, when he wrote his famous pages on ballistics. But although he devoted one page to 'ships of war', he chose not to include his thoughts about artillery in his paper on rigging.[43]

Conclusion

Harriot's interest in the culture of ordinary sailors certainly reveals a lot about his character. Stephen Clucas has shown that he collaborated with craftsmen at Syon

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[42] Cf. BL Sloane MS 3651, William Bourne on ordynance [1588?], ff. 31–2. For comments on this manuscript, see E.G.R. Taylor, 'A regiment for the Sea and other writings on navigation by William Bourne etc.', *Hakluyt Society*, LCXXI (1963), 24–5, 441–2. See also B. Caruana, 'Tudor Artillery, 1485–1603', *Historical arms series*, 30 (1992), Museum Restoration Service, Bloomfield, Ontario.

[43] Cf. BL Add. MS 6788, f. 31, 'Notes of shipes of warre'. Here, the author described the protections offered to sailors on such vessels ('top armores', 'trap hatches') and also flags and stremars used 'for gallantry'. On Harriot's ballistics, see S.A. Walton, *Thomas Harriot's ballistics and the patronage of military science*, Durham Thomas Harriot Seminar. Occasional paper n. 30 (Durham, 1999), 2–35; M. Schemmel. 'The English Galileo: Thomas Harriot and the force of shared knowledge in early modern mechanics', *Physics in perspective*, 8 (2006), 360–80 (also Schemmel's chapter in this volume); and J.-J. and P. Brioist, 'Harriot, lecteur d'Alvarus Thomas et de Niccolo Tartaglia', in J. Biard and S. Rommevaux (eds), *Mathématiques et théorie du mouvement, XIVe–XVIe siècle* (Lille, 2008), pp. 147–71.

House not only in the construction of scientific instruments but also in devising solutions to plumbing problems.[44] Clearly, he was also sufficiently familiar with the shipwright Mathew Baker to pass on to him his ideas about the dimensioning of masts.[45] It seems that his curiosity also extended to what he heard about fortification with Ralph Lane or about the 'precedence of souldiers', possibly with Ralegh or Northumberland.[46] As a matter of fact, the pages that follow those on rigging concern these issues.[47] On these two matters, Harriot's speculations are sometimes deeper than any to be found in the textbooks of his time, such as Roger Williams's *Brief discourse of war* (1590), Mathew Sutcliffe's *The practice, proceedings, and lawes of armes* (1593) or Paul Ive's *Practise of Fortification* (1589). On setting soldiers in array, for instance, he considered the idea of placing each pike-man in a square battle-order according to his particular qualities, giving small identifying numbers to courageous fighters and larger numbers to the less experienced footmen. His diagrams show that he believed that the best warriors should occupy corners or the middle positions in ranks and files. Although Mathew Sutcliffe in his treatise devoted some lines to the dignity of places, it was not before 1625, in Gervaise Markham's *Souldiers accidence*, that we find a convincing explanation of such sophisticated techniques.[48]

The same inventiveness can be recognized in Harriot's pages on fortification, strangely written in Latin, an exercise that forces him to use newly fashioned Latin words like *propugnaculis* (bastions) or *scabelli* (parapets).[49] Harriot's intelligence,

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44 S. Clucas, 'Thomas Harriot and the field of knowledge in the English Renaissance', in R. Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 93–136.

45 S. Johnston, 'Making mathematical practice: gentlemen, practitioners and artisans in Elizabethan England', University of Cambridge PhD thesis, 1994.

46 The best account on Harriot's military practice is found in Walton, *Thomas Harriot's ballistics*. Ralegh was a soldier who had fought in France and in Ireland, and Henry Percy wrote a number of pages on operations of the watch and annotated many books on warfare; see Alnwick MS W.11 1a–b and MSS of the Duke of Northumberland, vol. 8 Military Affairs, 1603.

47 See BL Add. MS 6788, ff. 50–70.

48 See G. Markham, *The souldiers accidence or an introduction into military discipline* (London, 1625); and Matthew Sutcliffe, *The practice, proceedings, and lawes of armes* (London, 1593).

49 See BL Add. MS 6788, ff. 54 and 56. One reads for instance on f. 54 the following explanation of a diagram: 'Ab et ac sunt duo latera inaequalia, ipud 70 hoc 50 perticarum (nam intra hoc limiter latera figuram irregularium coeteri debent, cum 60 perticae veram longitudinem obtineat, decem perticarum libertas ultro citroque permittitur). Muniendus est angulus a duobus dimidiis propu gnaculis adef et aghi, quorum hoc analogum sit minori lateri ac, illud majori ab. Divide latus ab biparium in L, et medietatem al transfer ab a ab m, eritq lm subbensa, pro inseni dividatur bipariam, et medietor a p transferatur ab a ad s, eritq ps subsenda cuius ope inveniuntur linea minori lateri ac congerua, minimum collum ag, capitatis ai et metam aq vel cr. Facier brevior hi producem dover abcindat ongiorem in

however, does not only concentrate on linguistic performance. His profile of a curtain and a ditch, for instance, is more elaborate than Paul Ive's.[50] It does provide dimensions for the rampier, the scarpe, the counterscarpe, the ditch and the parapets, but above all, it introduces an innovation with a '*promuralis*' (a fore-rampier) which is not often mentioned by military engineers. The folio entitled *munitio irregulari* is also fascinating, because instead of dealing with the general case, as Ive does, it deals with a situation in which there is a geometrical constraint: the point of the bastion is not lined up with the bisector of its angle.[51] At last, some of Harriot's diagrams show complex solutions to the problem of reinforcing a hexagonal fortress with scissors (*forceps*) and half-moons (*lunula*).[52] The idea of protecting a bastion with an advanced work composed of another bastion and two half-bastions might remind us of a solution adopted for the fortress of Doullens in Picardy, by Jean Errard, one of the best European engineers of the late sixteenth century. The historian Steven Walton has also suggested that the inspiration might have come from Samuel Marolois (1572–1627), a French engineer who served in the Netherlands and who was inspired by the Italian Francesco de Marchi.[53]

Of course, Harriot's thoughts remained completely theoretical, since England never considered spending the huge amount of money necessary for transforming such speculations into reality. We were some way ahead of the modest earthworks that Ralph Lane constructed on Roanoke Island! We are hence led to conclude that Harriot's mind, like that of Leonardo da Vinci, revealed a special capacity to absorb all sorts of practical knowledge. However, his writings do not show him to be a mere copycat of the ideas of others. On the contrary, he always imagined original solutions or considered unfamiliar cases. His contribution to the art of rigging a vessel was perhaps more modest. But, by the clarity of its pedagogy, his restless intelligence opened the way for later specialists who were to write their own treatise on the subject.

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k.' It is possible that these terms were borrowed from Giacomo Lanteri's Latin version of an Italian treatise on fortification entitled *Libri duo de modo substruendi terrena munimenta ad urbes atque oppida* (Venice, 1563), also issued identically as *De subtilitate ac stratagemate utenda in rebus bellicis ad destruendos hostes, necnon castra, eorum oppida fortissima* (Venice, 1571).

[50] BL Add. MS 6788, f. 56. Cf. P. Ive, *The practise of fortification* (London, 1589), published in 1972 as a facsimile by Gregg International Publishers Ltd, with the addition of an introduction by Martin Biddle on pp. 27ff.

[51] BL Add. MS 6788, f. 55.

[52] BL Add. MS 6788, ff. 59–64.

[53] S. Walton, 'Harriot, Lane, and fortification theory and practice', paper given at East Carolina University in May 2009 during the Thomas Harriot quadricentennial conference organized by Larry Tise.

Appendix A

The 'Perfect' Harriot/de Bry: Cautionary Notes on Identifying an Authentic Copy of the de Bry Edition of Thomas Harriot's *A briefe and true report* (1590)

Larry E. Tise

Thomas Harriot's pioneering contributions in geometry, mathematics, navigation, cartography and astronomy in the era of the English Renaissance are becoming widely known. But probably few, even among Harriot cognoscenti, are aware that he wrote what is considered the first detailed description of America to be published in the English language, i.e. his *Briefe and true report of the new found land of Virginia* (1588).[1] Or that this short, but sprightly narrative became the text for the first fully illustrated book on North America published just two years later in 1590. Or that it was published simultaneously in four languages: English, Latin, German and French. Or that this illustrated book became the first volume in the great European venture to publish illustrated books on most parts of the world unknown to Europeans prior to the age of exploration.[2]

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[1] *A briefe and true report of the new found land of Virginia: of the commodities there found and to be raysed, as well marchantable, as others for victuall, building and other necessarie uses for those that are and shal be the planters there; and of the nature and manners of the naturall inhabitants: discouered by the English colony there seated by Sir Richard Greinuile Knight, in the yeere 1585. which remained vnder the gouernment of Rafe Lane Esquier, one of her Maiesties Equieres, during the space of twelue monethes: ... by Thomas Hariot; seruant to the aboue named Sir Walter, a member of the Colony, and there imployed in discouering* (London, 1588). The titlepage is reproduced as Figure 1.1 in David B. Quinn, 'Thomas Harriot and the problem of America', in Robert Fox (ed.), *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 9–27 (p. 23).

[2] *A briefe and true report of the new found land of Virginia: of the commodities and of the nature and manners of the naturall inhabitants ... True pictures and fashions of the people in that parte of America now called Virginia. Som picture, of the Pictes which in the olde tyme dyd habite one part of the great Bretainne* (Francoforti ad Moenum: Typis Ioannis Wecheli, sumtibus vero Theodori de Bry MDXC. Venales reperiuntur in officinal Sigismundi Feirabendii, 1590). The Latin, German and French versions, of course, bore titles appropriate to those languages. For the Latin titlepage, see the frontispiece to this volume.

Even among historians of 'the book', it has not been recognized that during the four centuries since the publication of this illustrated version of Harriot's book in 1590 (which I will refer to here simply as 'the Harriot-de Bry'), there have been at least three periods when book-buyers and collectors sought avidly to find what they would call 'the perfect Harriot-de Bry'. In each of these three eras, separated roughly by a century or longer, what the would-be buyer meant when he went in search of a 'perfect' Harriot-de Bry was completely different. The purpose of this foray into the history of the Harriot-de Bry – one of the most influential books of modern history – is to shed some light on the colourful history of Harriot's remarkable work.

In the early spring of 2006, Joyner Library – the principal library at East Carolina University (ECU) – decided that it would be very appropriate for the University to own a good copy of the *Briefe and true report*, that is, of the 1590 first de Bry edition of the work. In fact, the inclination of all involved was that if the University were to acquire a copy, it should definitely be a 'perfect' copy, or at least the most perfect copy that could be found. Many impulses contributed to this consideration. One of them was that ECU had recently decided to give its college of liberal arts an historical name, that of Thomas Harriot. Following a lengthy and inspired campaign on the part of the college's Dean, Professor W. Keats Sparrow, the liberal arts core of the University henceforth became known as the Thomas Harriot College of Arts and Sciences.[3] Eschewing the prevailing American practice of placing donor names on virtually all limbs and branches of universities, ECU's trustees consented to Dean Sparrow's argument that the College should be named after an important historical figure.

Dean Sparrow chose this name to adorn the College partly because the historical Thomas Harriot was a significant but not fully appreciated Renaissance figure. Harriot had the other pertinent distinction that his feet had actually trod upon the soil of eastern North Carolina. On the second voyage of discovery to the coasts of North America organized by Sir Walter Ralegh in 1585, Harriot had been the scientist-in-residence who charted the waters from England to the Carolina coast, who assayed minerals and metals discovered on the mainland, who identified the

[3] I am very grateful that I came to know Dean W. Keats Sparrow (1942–2010) in the 1970s as he was rising to prominence through the English Department at ECU. My acquaintance with him resumed in 1999 when I was asked to serve as a consulting historian to North Carolina's First Flight Centennial Commission to commemorate the Wright brothers' many flights at Kitty Hawk, North Carolina between 1900 and 1911. It was partly through Dean Sparrow that I became associated with ECU and subsequently began researching Thomas Harriot, Walter Ralegh and the Roanoke voyages to the coast of North Carolina. It was a very sad day when Dean Sparrow, who had inspired so many souls at both ECU and across eastern North Carolina, died suddenly on 11 November 2010.

plants and animals encountered, and who – quite amazingly – prepared what is still a pretty accurate scale map of the Outer Banks of North Carolina.[4]

These were grounds enough to put Harriot's name on a school in eastern North Carolina. But beyond all of Harriot's other contributions was the fact that he had composed that first detailed description of North America written and published in English, his *Briefe and true report.* In actuality it was not just about North America or even Virginia, as was stated in its title. It was really about North Carolina, or what became North Carolina. Unlike any other American territory, there was in Harriot's book an examination of the flora and fauna, the waters and fishes, and its native Indian population and the way they lived. And not only was it about North Carolina, it was more particularly about what is now eastern North Carolina.

I knew very little of this background when I volunteered to assist Thomas Harriot College and Joyner Library at ECU figure out how to evaluate copies of the publication that might come on the market and be available for purchase. By the time I arrived on the scene, my colleagues at Joyner Library had already rejected a couple of copies because they were obviously defective: missing pages, photocopied inserts, damaged condition, defaced engravings and the like. They had also consulted some of the standard digests describing the characteristics of early books printed in Europe and the USA. Ancient and respected booksellers had a penchant for writing detailed descriptions of books they had seen and sold, including the Harriot-de Bry editions.[5] They had also consulted W. John Faupel's 1989 compilation of Harriot-de Bry printings and editions. Described in its title as *A study of the de Bry engravings*, Faupel – a dealer in maps and de Bry engravings – had produced an impressive digest of title-pages, engravings and many details on various editions and states of the Harriot-de Bry. These indicators could be used to determine with great precision when a particular book had been produced in the 40 or so years that the de Bry company was printing these volumes.[6]

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4 There are many discrete essay studies of Thomas Harriot, especially of his long list of research interests. His biographies, however, have been mainly odd and worshipful works: Henry Stevens, *Thomas Hariot, the mathematician, the philosopher, and the scholar. Developed chiefly from dormant materials, with notices of his associates, Including biographical and bibliographical disquisitions upon the materials of the history of 'Ould Virginia'* (London, 1900) – a somewhat hagiographical and exaggerated account; Muriel Rukeyser, *The traces of Thomas Hariot* (New York, 1971) – a somewhat mystical reverie on Harriot's earthly existence; and John W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983) – a polemical work attempting to document Harriot's priority in various scientific discoveries and mathematical calculations.

5 Among the standard book references with extensive notes on Harriot-de Bry volumes are: Elihu Dwight Church, *A catalogue of books relating to the discovery and early history of North and South America*, 5 vols (New York, 1907); Charles Evans, *American bibliography*, 13 vols (Chicago and Worcester, MA, 1903–55); and Joseph Sabin, *A dictionary of books relating to America*, 29 vols (New York, 1868–1936).

6 W. John Faupel, *A brief and true report of the new found land of Virginia. A study of the de Bry engravings* (East Grinstead, 1989).

But Faupel's book looked at the numerous editions of Harrot-de Bry that emerged over many years – not just those printed in 1590 – and was thus of limited use for our purpose. We were interested in acquiring a first edition and first state of the work that was produced by de Bry in 1590 in the first run of Latin, German, French or English copies. Moreover, Faupel's book only looked at printing characteristics and ignored the condition, completeness, bindings, trim, cleanliness, conservation treatments and other variants that need to be known in the process of evaluating the intrinsic or historical value of particular copies of the book.

Coterminous with the emergence of a desire to acquire a copy of the Harriot-de Bry, a few copies appeared on the market. One was sold at an auction in North Carolina. Another was offered by a bookseller in New York. Other copies of various descriptions were available from a few booksellers scattered across Europe. But all of these copies had certain 'imperfections', which were usually acknowledged in the listings. A missing map, torn pages, photocopied inserts, broken bindings, marked or defaced pages – the list of obvious defects went on and on. Even in broken and imperfect form, the prices of these versions seemed enormous, ranging from $30,000 to $400,000, depending on a slew of factors, including missing engravings on the low end to bindings that included other copies of de Bry's engraved books on the high end. All of these imperfect copies were easily discounted, dismissed and rejected.

But suddenly there appeared a very interesting copy of the 1590 Latin edition that had been in the hands of a Virginia bookseller for many years. Our librarians became enthusiastic about this copy because it seemed to be complete. Further, the book had a previous association with a well-known, meticulous and fabled book antiquarian from western North Carolina. The price was hefty, but it was available to be purchased and placed in the rare book collection of Joyner Library if we wanted to invest a sum of money greater than for any other book ever before purchased by the University. We were talking about the kind of money that would embarrass a librarian or a university if the book turned out in the end to be a fake or a fraud, or stolen or somehow less than perfect. Since the money in question was ultimately public money – even if from an endowment (ECU is a public university and part of the grand University of North Carolina system) – we needed to be careful. No-one wanted to wake up one day and read in the press that this public university had been somehow ripped off, whether by deceit, misfeasance, nonfeasance, malfeasance or sheer stupidity.

Our librarians wisely got the book on approval in order to examine it and to go over it with a fine tooth-comb. As the acquisition of such a gem was one of the fondest goals of Dean Sparrow, he asked to be present when this copy was taken out of its shipping carton. He was there. He exclaimed when he saw it. He thumbed through its pages (with white gloves) and proclaimed that from his point of view, the money ought to be spent to acquire the book before it might find its way into other less appreciative hands. It looked real. Authentic. Beautiful. And our librarians agreed.

When Dean Sparrow shared with me the dilemma of whether to buy or not to buy this particular Harriot-de Bry, I also shared my concerns that a great opportunity should not be missed. Naively, very naively, almost quixotically, I volunteered to see what I could learn about the book. From the first moment I leafed through the book, I began to see the dilemma. Although I had spent years studying the contents of rare books and manuscripts, I was a mere novice when it came to evaluating the nature and authenticity of this particular book. I saw almost immediately why the librarians were cautious. And before I ended my Harriot-de Bry pilgrimage, I would learn just how complicated it is to determine what is a 'perfect' copy.

At various stages of my career as historian, I have been an exponent of and practitioner in the study of comparative history. We compare slavery in different colonial empires, revolutions in various nations and peoples in separate cultures. So wouldn't it be useful to compare different copies of Harriot-de Brys? To me, this seemed like the best and quickest way to gain some knowledge on how to identify a 'perfect' Harriot-de Bry. And so my pilgrimage began.

I did not have to go very far to get my first comparative example. It happened that Hope Plantation in Bertie County, North Carolina (not far from ECU) had purchased its own Latin edition of Harriot-de Bry and its copy was temporarily on deposit at Joyner Library. I thus sat down with the two books – both in Latin – and began turning pages simultaneously in both copies. My first set of lessons immediately began.

Although both books were supposed to be authentic copies of the 1590 Latin edition of Harriot-de Bry, the two of them were as different as night and day. The Hope Plantation copy was, first of all, neat and clean, almost as if it had never before been touched. 'Our Copy' (as I began to call it, long before the University had made a decision to buy it), by contrast, was dirty. The pages showed the grime of many dirty fingers; the Hope Plantation copy, none. The Hope Plantation copy was neatly trimmed, with its pages gilt-edged, whereas Our Copy had uneven page edges – no fancy gilt. The Hope Plantation copy had been very tightly rebound in a fancy leather case. Our Copy had been rebound too, but not with the machine-like tightness and precision of the Hope copy. On first impression alone – due to cleanliness, beautiful binding and gilt – one is perhaps almost automatically driven to favor the neater and more compact book as being the more nearly 'perfect'.

As I began comparing the pages of the books side by side, however, I noticed that those of the Hope Plantation copy had slightly different colours and textures as one proceeded from page to page. This was not the case with Our Copy. Its pages were uniformly dirty, off-white and felt pretty much the same to bare fingertips (the gloves had to come off for this analysis). I also noticed that there were also differences in the sequence of pages, sections and features of the two books. The separation between engravings and print on what should have been identical pages was different in the two books. Some of the initial letters in chapters or the captions for engravings were different, as were print lines on some pages of the two books. Some of the headplates and footplates were different. How could

the two books, despite bearing titlepages saying that they had been produced by Theodor de Bry and published by Sigmund Feyerabend in Frankfurt in 1590, be so completely different?

From Joyner Library I went next to the North Carolina Collection at the University of North Carolina in Chapel Hill, the fanciest nest in the world for researching early Carolina history. This fine collection was established in the early twentieth century with a goodly stock of North Carolina imprints. Its goal is to collect everything published about North Carolina or by North Carolinians. The Collection abounds with several variations of the 1590 Harriot-de Bry in Latin, but it also has a German 1590 edition. And what a remarkable version this German edition turned out to be. It was beautifully and heavily hand-coloured. Every

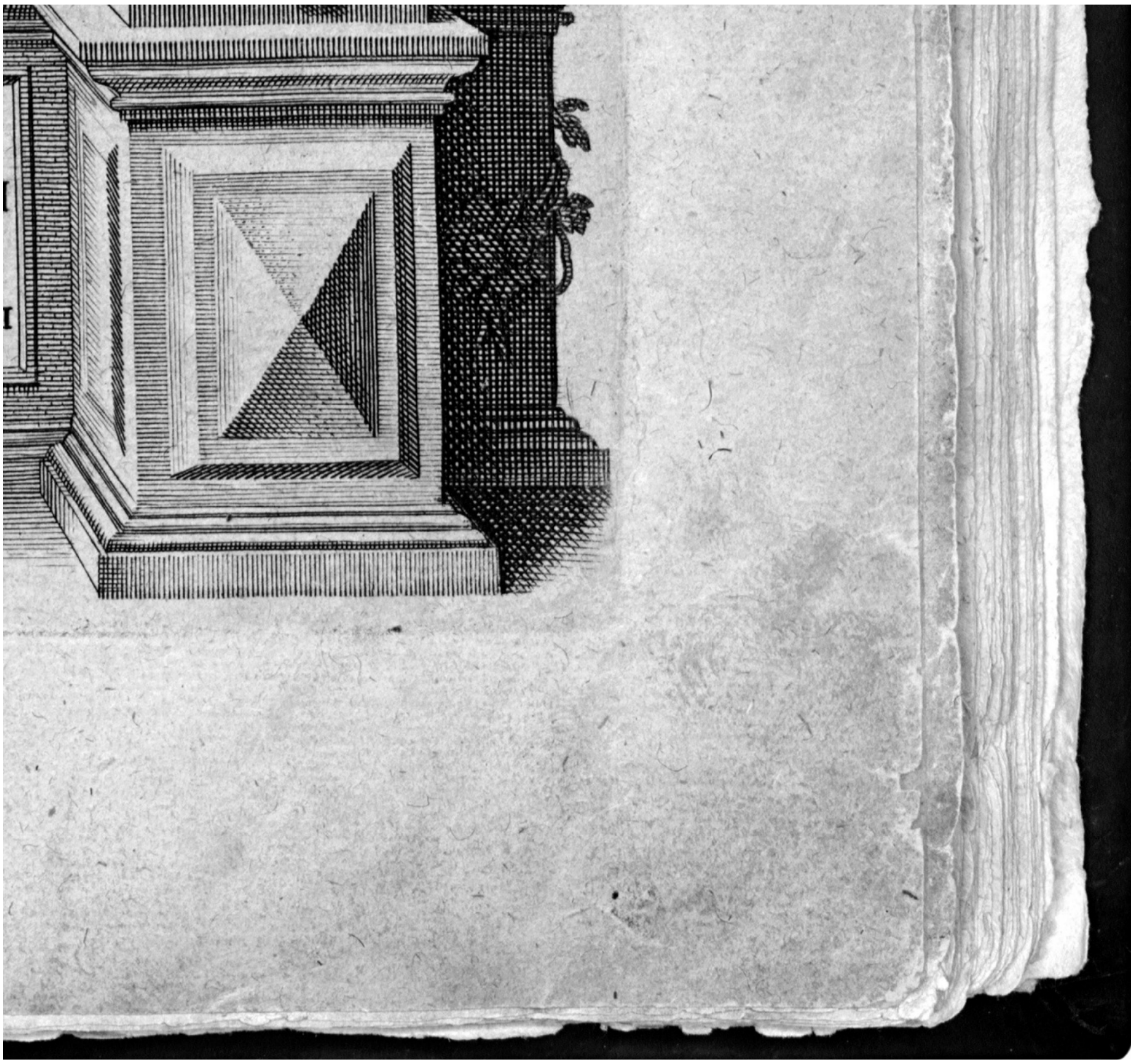

Figure A.1 Detail of lower right-hand corner of the title-page of *Admiranda narratio*, showing effect of use. By permission of the Rare Book Collection, J.Y. Joyner Library, East Carolina University, Greenville, NC, USA

engraving was in bold colours. Headplates and footplates had been coloured. Adam and Eve in the middle of the book, due to the colouring, looked remarkably Germanic. And so did some of the Indians. The Picts at the back of the book looked garish enough to be in a Hollywood movie.

When I saw this German version – hand-coloured to boot – I began wondering where other copies of Harriot-de Bry might be located and where one might find other hand-coloured versions as well. With the help of Nicholas Graham, a librarian at the North Carolina Collection, I began looking at online guides to library holdings. We quickly found that many libraries, particularly the older university libraries, had copies, even multiple copies of Harriot-de bry. But there were no other hand-coloured versions in these online guides. There also seemed to be a great variety of ways of cataloguing Harriot-de Brys. 'Harriot' was often to be encountered as 'Hariot', sometimes the catalogue listing would be under de Bry and sometimes simply under its Latin, German, French and English titles.

One library popped out as of special interest to me. The Garrett Library at Johns Hopkins University had four complete versions of the 1590 book: Latin, German, French *and* English. What a bonanza or potential bonanza! I could to go to Johns Hopkins in Baltimore and in one spot on one day do a comparative study of all four of the languages in which the book was published. John Buchtel, a Johns Hopkins rare book librarian, arranged to meet me at the Garrett Library. Administered by but not consolidated into the principal university library at Johns Hopkins, the Garrett Library remains in the last residence of the Garrett Family, Evergreen, in Baltimore. John Work Garrett (1820–84) accumulated a fortune in banking and railroading, enabling his family to acquire a vast historical library containing some of the choicest rare books. Collected during the era of the Civil War and later by his son T. Harrison Garrett and grandson, also named John Work Garrett, the Garrett Library was and remains one of the prototypical great private libraries assembled through wealth in the nineteenth and early twentieth centuries. This was a popular American pursuit in the period that historians often call the 'Robber Baron' era. It was a practice I had noticed before. But it had never occurred to me, not even on the day I went to the Garrett Library, that there were certain given formulae that were used all over to create genteel personal libraries of this nature.

John Buchtel was a godsend for me. He met me at the Garrett in the morning and stayed with me for the entire day. He brought out the four Harriot-de Brys so that I could look at them one by one. But when I told him I should prefer to see all four at the same time, he generously allowed me to turn forward and back as I wished through the four books simultaneously. This allowed me to see both the major and the minor differences occasioned by the use of separate languages. The Latin was shorter than the others and the German the longest – several pages longer. The Latin typeface had a clean, simple, even terse look. The German characters were dense and florid by comparison. The French type characters seemed flowing and beautiful. And the English, like all texts of the time, had a jumbled, inconstant,

somewhat bumpy appearance. German characters were dense and dark, the others thin sticks.

There were also marked dissimilarities between the headplates and footplates in the different editions. The initial letters in each chapter or in each caption differed from language to language. The dedication pages were different in each language edition, as de Bry selected particular patron-figures in Germany, England and France to give honour to his work. Each edition had a slightly different titlepage, not in basic design but rather in the notation of publication data. Another obvious difference was that the books were of different lengths and some of the engravings were very crowded by the printed texts in different languages.

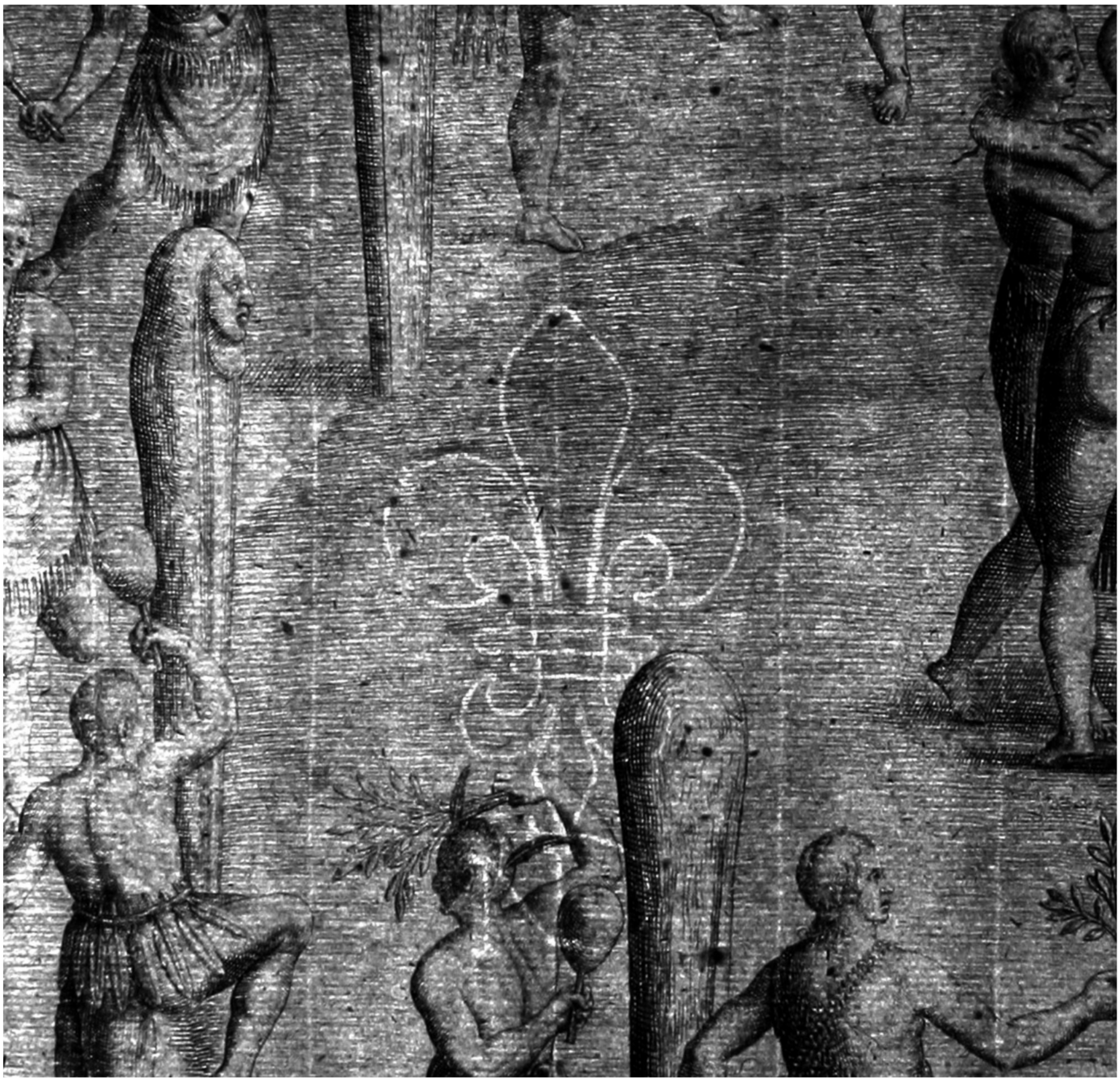

Figure A.2 Detail of Plate XVIII of *Admiranda narratio*, showing watermark and chain marks. By permission of the Rare Book Collection, J.Y. Joyner Library, East Carolina University, Greenville, NC, USA

However, with John Buchtel's help, I noted things about the four books that were the same. All of them had the same kind of paper. Not that the colours of the paper were always consistent. Rather, by looking closely at the paper, it was evident that all of these copies were made from paper made on the same machines and thus by a single paper-maker. The chain marks of the paper machine were the same in all four copies, giving the paper, upon close examination, the look of a fine woven fabric. And the chain marks had a particular width, roughly three centimetres.

All of these copies had something else in common: the same watermark. Paper-makers of that period had different watermarks to distinguish their papers. They did then and still do, if the papers are fine and have cloth or cotton fibers in them. All four of the books had the same watermark. The watermark had three sections to it: the top part is in the shape of a fleur-de-lis; the middle part a rectangular box; and the bottom part bears the distinctive letters 'ND'.

Another point of similarity was that the book in each language had the same parts, although some of the parts might be in a slightly varying arrangement or order. All of the language variations contained the following elements:

- Titlepage.
- Publisher's note by Theodor de Bry.
- Dedication.
- The text of Harriot's *Briefe and true report.*
- An engraving of Adam and Eve.
- A total of 23 engravings – two maps of the Carolina coast and the rest depictions of the appearance and way of life of the Indians who lived near the Island of Roanoke; all of these (except the first map) were subjoined by lengthy captions composed by Harriot.
- Five additional engravings depicting the early inhabitants of England called the Picts who dressed, tattooed and coloured themselves in some of the same manners as the Indians around Roanoke; the captions here were evidently written by Theodor de Bry.
- An index of terms and subjects covered in the book.

It became clear to me that for any copy of Harriot-de Bry, irrespective of the language, to be considered perfect, it had to have not just some, but all of these component parts.

It was also evident that there were many different ways of folding, assembling and binding the parts of the books. Some of the engravings are larger than one page. These might be folded into equal parts and sewn into the book at the gutter of the page. Others might be folded, trimmed and tipped with an additional strip of paper into the gutter. Still others might be left untrimmed, folded and tipped, but with an additional fold of that part of the paper that was larger than the book itself. And because there were many ways of printing and folding and binding the books, one would not necessarily find the watermark on the same pages in all of the

Table A.1 Copies of the 1590 edition analysed by language and owner institutions

Language	Owner institution	Holding division	Location
English	British Library		London, UK
English	Free Library	Elkins Collection	Philadelphia, PA, USA
English	Johns Hopkins University	Garrett Library	Baltimore, MD, USA
English	Library of Congress		Washington DC, USA
English	Oxford University	Bodleian	Oxford, UK
English	Oxford University	Bodleian	Oxford, UK
English	Oxford University	Bodleian	Oxford, UK
English	Princeton University	Firestone Library	Princeton, NJ, USA
French	Bibliothèque nationale	Mitterand	Paris, France
French	Free Library	Elkins Collection	Philadelphia, PA, USA
French	Harvard University	Houghton Library	Cambridge, MA, USA
French	Johns Hopkins University	Garrett Library	Baltimore, MD, USA
German	Free Library	Elkins Collection	Philadelphia, PA, USA
German	Johns Hopkins University	Garrett Library	Baltimore, MD, USA
German	UNC Chapel Hill	NC Collection	Chapel Hill, NC, USA
Latin	Bibliothèque nationale	Arsenal	Paris, France
Latin	Bibliothèque nationale	Mitterand	Paris, France
Latin	Bibliothèque nationale	Mitterand	Paris, France
Latin	Bibliothèque nationale	Mitterand	Paris, France
Latin	Bibliothèque nationale	Mitterand	Paris, France
Latin	Bibliothèque nationale	Arsenal	Paris, France
Latin	Bibliothèque nationale	Arsenal	Paris, France
Latin	British Library		London, UK
Latin	Brown University	John Hay Library	Providence, RI, USA
Latin	East Carolina University	Joyner Library	Greenville, NC, USA
Latin	Free Library	Elkins Collection	Philadelphia, PA, USA
Latin	Johns Hopkins University	Garrett Library	Baltimore, MD, USA
Latin	Mariners' Museum		Newport News, VA, USA
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	National Maritime Museum		Greenwich, UK
Latin	Oxford University	Christ Church	Oxford, UK
Latin	UNC Chapel Hill	NC Collection	Chapel Hill, NC, USA
Latin	University of Pennsylvania	Van Pelt Library	Philadelphia, PA USA

books. The watermarks were almost always contained in a regular and consistent pattern in each of them. But the pages on which the sequence commenced could be different for each book. It was especially pleasing to find watermarks on titlepages and also on the engravings. Often the watermarks showed up as ghost marks on adjoining pages that have been in perpetual contact with these pages – invariably a good sign as to the original character of a copy of the book.

Due to what I had learned at the Garrett Library, I felt ready to take on the world of Harriot-de Bry books. Over the next five or six months I was able to examine a great many additional copies of the 1590 Harriot-de Bry at some of the greatest libraries of the world. Paris, London, Rome, Oxford and Cambridge were on my list, as were Washington DC, New York, Boston and other places in the USA. It so happened that I was on my way to a series of meetings and research libraries that enabled me to add to other activities an examination of such copies of the Harriot-de Bry as I might encounter.

Building on my preliminary analyses of the copies at ECU, Chapel Hill and Johns Hopkins, I developed an array of data-queries that I decided to put uniformly to every copy of the book I might encounter.[7] I determined that I would go through each copy, cover-to-cover and page-by-page, and thus figure out how these books were arranged. In addition to recording information about the holding library and its location, I also decided to track a minimum set of data about each copy of the book. These pieces of information were the minimum, because I ended up recording much more information about each copy of the book, so much that I came to know each one quite personally. I would easily recognize each of them whenever I saw it again.

Over six months in 2006 I examined more than 70 copies of the Harriot-de Bry publication, covering all four languages and in a widely spread set of institutions. I also made a special effort to visit places that had multiple copies of Harriot-de Bry. I was especially interested in places that had English and French copies, due to their rarity. And I was interested in any places that might have collected copies of all four languages. Most of the copies I examined were from the 1590 original editions of the work. I ended up collecting a fairly complete set of data on 38 copies, the ones that I have chosen for inclusion in this study.

The institutions whose copies are included are the following:

- Bibliothèque nationale, Paris (8 copies).
- British Library, London (2).
- Brown University (1).
- East Carolina University (1).
- Free Library of Philadelphia (4).
- Harvard University (1).
- Johns Hopkins University (4).
- Library of Congress (1).

[7] These queries and data recorded are listed later in this appendix.

- Mariners' Museum, Newport News, VA (1).
- National Maritime Museum, Greenwich (7).
- Princeton University (1).
- University of Oxford (Bodleian 3; Christ Church 1).
- University of North Carolina at Chapel Hill (2).
- University of Pennsylvania (1).

It is not suggested that these institutions necessarily have the best copies of Harriot-de Bry. But they are so nicely scattered and represent such a variety of collecting interests and styles over such a long period of time that they turned out to be ideal for my purposes.[8]

In addition to having a nice spread of institutions, both historically and geographically, I also achieved a pretty good spread of the four language editions. All of the books bore the 1590 imprint, but they were divided among the four languages as follows:

Latin	23 copies
English	8
French	4
German	3

Although this grouping tended to emphasize the rarer English and French editions over the more abundant German editions, one of my special interests was to see how the English editions stacked up against the others.

Another factor was whether the Harriot-de Bry editions were published and bound as individual volumes or whether they had been originally published and bound or perhaps later bound together with other DeBry volumes. The book we were examining at ECU was a stand-alone volume, so I wanted to know as much as possible about these lone soldiers. The binding formats of the copies I examined were as follows:

Multi-volume	14
Stand-alone	22
Manuscript	2

When the various Harriot-de Bry copies appeared in a multi-volume format, they were usually to be found in variously titled and bound editions. Among the spine titles they might bear were as follows: Admiranda, Historia Americae, Admiranda Narratio, Americae Occident, Brevis Narratio, Peregrina in Indiam Occidental, India Occidentalis and Grands Voyages. Another binding characteristic that

8 While it might have been nice to include more copies of the book in this study, it is unlikely that adding an endless number of copies would have affected the major results of the study

caught my attention and proved to be important in identifying perfect Harriot-de Brys was the treatment of page edges when these books were bound. Some of the books had perfectly beautiful gilt edges. Flipping through one of these books was a little like putting your thumb on the soft edges of a King James version of the Bible. Sometimes I encountered marbled edges that gave the same smooth effect, but just as many of the books had plain edges without any special treatment or effect. Of the 38 copies I examined, 17 were gilt, one was marbleized and the rest had plain edges.

If the books were gilt or marbleized, this meant that they were carefully and neatly trimmed so as to produce the dramatic smooth edges of such treatments. I also took notice of how much the edges of pages had been trimmed to even up the pages. Almost all of the books had been trimmed at least to some extent to make the pages sufficiently even to be turned with bare fingers. Some had been trimmed severely to make a neat and compact, very modern-looking book. Some clearly had been trimmed more than once as they went through various rebinding experiences to fit in one library or another. A very few had uneven, almost floppy page-edges, suggesting that they were still in the pristine form in which the book had come into existence. While a lot of trimming might make a pretty book, I soon came to prefer those that had some of the fluffy edges of the original papers used to make the books.

The wide disparity in the trimmed appearances of these books suggested that one measure that might help determine which books were the most pristine and undisturbed might be the present height and width of the books' pages. To test this theory, I began measuring the pages of every book I touched. Top to bottom was easy to accomplish. Side to side was more difficult, but I sought the deepest trough for the left edge of my measurement or, where the binding permitted, the observable paper's edge. As eventually became apparent, this measure of the height and breadth of the pages, multiplied to determine geometric surface area, became the quickest measure of calculating the probable pristine quality of any given copy of a Harriot-de Bry publication.

Somewhat astonishingly, the trimmed and bound height of these books ranged between 35 cm and 32.4 cm, while the width ranged from 24.8 to 22.7 cm. Expressed in squared terms, the resulting disparity ranged from 858 sq cm on the largest size to a minimum of 699 sq cm. This manifest disparity became for me the surest sign of severe interventions to change the character and appearance of the book from what it might have been when it was originally created to what it had become by the time it was put before me for inspection.

At about the same time as I began taking what I thought were sure to be telltale measurements of pages, I began noticing another factor that seemed to be closely correlated to the relative size of the page trim. Some of the books' pages were exceedingly dirty, while others were exceedingly clean. The dirty ones often showed signs that the lower right-hand corner of pages had been turned many, many times leaving an easily detectable measure of finger grime and dirt. Even fingerprints were evident on some pages. Other copies were so noticeably clean

Table A.2 Copies of the 1590 edition analysed by language, binding format, paper condition, treatment, colour and page edges

Language	Binding format	Paper condition	Paper treatment	Paper colour	Page edges	Owner institution
English	Stand-alone				Gilt	Princeton University
English	Multi-volume	Dirty	Unwashed		Gilt	Oxford University
English	Stand-alone	Dirty	Unwashed	Off-white	Gilt	Free Library
English	Stand-alone	Dirty	Unwashed	Off-white	Plain	Oxford University
English	Manuscript vol	Dirty	Unwashed	Off-white		Oxford University
English	Stand-alone	Untouched	Unwashed	Off-white	Gilt	British Library
English	Stand-alone	Clean		Off-white dark	Plain	Johns Hopkins University
English	Stand-alone	Clean	Washed	White	Plain	Library of Congress
French	Multi-volume	Dirty		Dark	Plain	Johns Hopkins University
French	Multi-volume	Dirty	Unwashed	Discoloured	Gilt	Free Library
French	Stand-alone	Dirty	Unwashed	Off-white	Plain	Harvard University
French	Stand-alone	Very Clean	Unwashed	Off-white	Gilt	Bibliothèque nationale
German	Multi-volume	Dirty		Dark		Johns Hopkins University
German	Multi-volume			Off-white	Plain	Free Library
German	Stand-alone	Very Dirty	Unwashed	Off-white	Plain	UNC Chapel Hill
Latin	Multi-volume		Unwashed		Marbleized	British Library
Latin	Stand-alone	Clean	Unwashed	Dark	Gilt	Bibliothèque nationale
Latin	Multi-volume	Dirty	Unwashed	Dark	Plain	Brown University
Latin	Multi-volume	Dirty	Unwashed	Dark	Plain	Free Library
Latin	Multi-volume	Clean		Off-white	Plain	Johns Hopkins University
Latin	Multi-volume	Clean	Unwashed	Off-white	Gilt	Oxford University
Latin	Multi-volume	Dirty	Unwashed	Off-white	Gilt	Bibliothèque nationale

Language	Binding format	Paper condition	Paper treatment	Paper colour	Page edges	Owner institution
Latin	Stand-alone	Dirty	Unwashed	Off-white		Mariners' Museum
Latin	Multi-volume	Dirty	Unwashed	Off-white	Plain	UNC Chapel Hill
Latin	Stand-alone	Dirty	Washed	Off-white	Plain	University of Pennsylvania
Latin	Multi-volume	Very Clean	Unwashed	Off-white	Plain	Bibliothèque nationale
Latin	Stand-alone	Very Clean	Unwashed	Off-white	Gilt	Bibliothèque nationale
Latin	Stand-alone	Very Clean	Unwashed	Off-white	Plain	Bibliothèque nationale
Latin	Stand-alone	Very Clean	Unwashed	Off-white	Plain	Bibliothèque nationale
Latin	Stand-alone	Very Clean	Unwashed	Off-white	Gilt	Bibliothèque nationale
Latin	Stand-alone	Very Dirty	Unwashed	Off-white	Gilt	East Carolina University
Latin	Stand-alone	Clean	Washed	White	Gilt	National Maritime Museum
Latin	Stand-alone	Clean	Washed	White	Gilt	National Maritime Museum
Latin	Stand-alone	Clean	Washed	White	Gilt	National Maritime Museum
Latin	Stand-alone	Clean	Washed	White	Gilt	National Maritime Museum
Latin	Stand-alone	Clean	Washed	White	Gilt	National Maritime Museum
Latin	Multi-volume	Clean	Washed	White	Plain	National Maritime Museum
Latin	Stand-alone	Clean	Washed	White	Plain	National Maritime Museum

that they looked as if they had just been opened for the first time ever. No dirt, no grime, no fingerprints. A book so clean that one might feel that it just came out of a printer's shrinkwrap.

The dirt and cleanliness of various copies also seemed to go hand in hand with another characteristic that I could detect across the array I sampled. Feeling the paper between my bare fingers, I detected that some leafs were stiff and others quite limber; some had a roughness of the finest sandpaper, while others had the quality of soft tissue paper. All of the papers in any particular copy might be of one quality or the other. Or, perplexingly, these types of paper might be mixed and matched in the same book. How could this mixture and matching have happened? Would this have been done in the de Bry print shop? Or could it have happened when various souls began remodelling the book up for sale to someone else?

Hand in hand with these observations came others that seemed to be similarly related. I noticed that the paper in some of the books seemed to be exceedingly white, so white in fact that the papers in one copy could not have come from the same source as the papers in another copy. The paper ranged from being exceedingly white to variations of off-white all the way to having a dark cast, even though through other measures I could determine that all of these papers came from the same paper manufacturer and bore the same manufacturing characteristics.

As I was going about examining copies of the Harriot-de Bry, I was also checking the bibliographic data surrounding many of the copies I saw. I was hoping to find some evidence relating to the origins of individual copies. Unfortunately, many libraries had very little information about provenance. In the European libraries, most of the acquisition records were buried in the mists of time. The books were in the libraries when a particular inventory was done in the nineteenth or early twentieth century. Others had been donated by alumni and friends of the libraries in the twentieth century. And frequently the donors had little information about the books before they purchased them from one dealer or another.

At a few of my stops, however, I got some vital insights into the question of provenance. For example, when I made my way to the Houghton Library at Harvard University to examine its magnificently hand-coloured French copy of Harriot-de Bry, Thomas Horrocks, whom I had previously known at the Library of the College of Physicians in Philadelphia, pulled out the files relating to that elegant copy. Contained in the file was an exchange of letters between the librarian of Harvard College and Henry Stevens, a bookseller, who styled himself as being 'of Vermont', even though the address of his bookstore was 4 Trafalgar Square in the heart of London! According to the letters in this file, Stevens agreed in 1870 to help Harvard make its copy of Harriot-de Bry 'complete'. He agreed to provide a 'genuine *original*' titlepage. And he agreed to provide 'facsimilies well done' of the texts to accompany the Harriot-de Bry plates numbered IIII, V, VI, VII, X, XIII, XVI, XVIII and XX. These facsimiles, as the agreement had it, 'are to be

delivered as soon as they can be prepared after my return to London'. In other words, for certain considerations of money, Stevens would help make Harvard's somewhat defective version of Harriot-de Bry *perfect*.[9]

This correspondence came at a crucial time, since it enabled me to become more deeply acquainted with a unique character whose career would lead me to a completely new and alternative understanding of what was a 'perfect' Harriot-de Bry. I already knew the name of Henry Stevens from his curious little biography of Thomas Harriot published posthumously by his son and book-business heir, Henry N. Stevens, in 1900. The title of this little book, which first popularized the career and achievements of our principal subject, was *Thomas Hariot, the mathematician, the philosopher, and the scholar*. The biography was described as one of the loving projects written during his lifetime by 'Henry Stevens of Vermont' and it was 'developed chiefly from dormant materials … including biographical and bibliographical disquisitions upon the materials of the history of "Ould Virginia"'.[10]

For Henry Stevens, who was the consummate dealer in Harriot-de Bry editions, the 'perfect' copy of the book was one that had a complete set of pages in a single language and one in which each and every page, seen alone, had to be a perfect page. It mattered little to Stevens that the individual pages could have come from many different editions and copies of Harriot-de Bry. It only mattered that each and every page had to appear to be authentic. Therefore, in order to create what he called perfect copies, Stevens collected loose pages from whatever source he could find. Not only loose pages – he also bought up damaged and incomplete copies everywhere he travelled so that he could scavenge from those copies pages as needed to make up new complete books that he could then sell as 'perfect' copies.[11]

A classic case of how he went about doing this was with the renowned book collector James Lenox of New York City, whose phenomenal library eventually became the core collection of the New York Public Library. According to Stevens's recollections, he once acquired a copy of Harriot's 1588 quarto edition of the *Briefe and true report*, the rarest of all exploration books. But the small quarto was missing four leaves. These missing pages, Stevens later recalled:

[9] Memorandum of receipt to John L. Sibley, Librarian of Harvard College and Henry Stevens, 11 April 1870. John L. Sibley to Henry Stevens, 1 November 1875. Justin Cousin, Harvard College Library, to Henry Stevens, 17 December 1877. Henry Stevens to Dear Sir (Justin Cousin), 16 October 1879. Houghton Library, Harvard University.

[10] See note 4 above for the full title of Stevens's book.

[11] I began corresponding in early 2011 with Ken Gore of Haslemere in Surrey, who worked from 1969 to 1985 at the Henry Stevens bookstore in Farnham. In an email of 16 February 2011, Mr Gore confirmed that even as late as 1985 when the Stevens book operation was moved from England to Williamsburg, VA, the store possessed many 'broken' copies of Harriot-de Bry which were used for 'perfecting' copies of the book which were then also 'properly described' as 'perfect' copies.

> I had very skillfully traced by Harris, transferred to stone, printed off of old paper of a perfect match, the book and these leaves sized and coloured alike, and bound in morocco by Bedford. The volume was sent to Mr. Lenox to be examined by him *de visu*, the price to be £25; but if he could detect the four fac-simile leaves, and would point them out to me without error, the price was to be reduced to £21.

Although Lenox tried to identify the four substitute pages, according to Stevens, he failed and thus had to pay the full price.[12] But putting in substitute pages was just one of the practices followed by Stevens. By washing the pages, old and new, by trimming and rebinding, by putting gilt edges on the pages and by intruding other alchemical and technical procedures in reformatting his copies of Harriot-de Bry, he was able to offer, even to the most discriminating client, what seemed to be perfect books.

These limited sources on the special life and career of Henry Stevens were enough to show that there existed in the nineteenth and early twentieth centuries a completely alternative view of what constituted a 'perfect' Harriot-de Bry. But I was hardly prepared for the intensive education that awaited me at the National Maritime Museum at Greenwich. Having learned from John Faupel's book that the Museum owned a large collection of the books, I was curious as to why this particular museum might own so many copies. Although Faupel had examined the 21 copies at the Museum in compiling his study, he did not explain why it had so many of them.[13]

I requested ahead of time to see all of the copies of Harriot-de Bry when I made my two-day visit to Greenwich in late 2006. As happens in many libraries with rare and special collections, the librarian assigned to deal with me told me politely that the Museum's policy was to permit a researcher to have only one rare item at a time on his or her desk. 'But I need to see them all – and at the same time', I pleaded beforehand and in person when I arrived. Maybe it was my continued insistence or the fact that I was so far from home that won the day. When I arrived at the Museum, all of the books were carefully placed on a single library truck for me to examine any or all of the copies as I liked. Before the day was over, I had all of their 1590 editions in both Latin and German arranged side-by-side on my researcher's desk.

Even before I had all of the books out on a desk – indeed, as soon as I saw all of them resting side-by-side in the library truck – I could tell that I was in the presence of probably the largest number of 'perfect' copies of Harriot-de Bry that

12 Henry Stevens, *Recollections of James Lenox and the formation of his library* (New York, 1951), pp. 110 and 113. According to the editor of this volume, Victor Hugo Paltsits, the Stevens story does not conform precisely to the facts of the case. Paltsits wrote that he could easily find six substitute pages when he looked at it around 1950 (p. 113).

13 Faupel, *A brief and true report*, pp. 6n. and. 85n., does mention that Henry Stevens, Son and Stiles of Farnham, Surrey did catalogue and sell at least one copy of Harriot-de Bry.

would ever be assembled in one place and at one time. Before I opened a single copy, I wondered if I had not somehow stumbled upon the treasure trove of Henry Stevens's private collection of unsold copies of the book. All of the books had the same beautiful leather binding with impressive gold lettering. All had neatly trimmed and gilt pages. All of them felt as if they were entirely new books. They opened stiffly just as would a new book with a tight binding and heavy papers. Inside, the pages were uniformly clean and very white. The leaves were stiff and rigid, almost sandpapery to the touch.

I was transported back to the day I examined side-by-side Our Copy and the Hope Plantation copy at ECU when I first set out on this venture. Although all the Maritime Museum copies bore the date of 1590 and were either in Latin or German, that was the end of the similarities between and among them. As I turned the same pages in all the copies in simultaneity, the vast differences and disparities among them became starkly obvious. The titlepages were virtually all different. The arrangement of pages was different. Dedication pages seemed to have been gathered from different places. Watermarks seemed to have no particular consistency. Initial letters for chapters and engravings were often different. Some papers did not have standard chain marks. Printed pages in some copies looked as if they had been culled from different books. Some engravings were sewn into the bindings. Others were tipped into the gutter in a variety of haphazard manners. Yes, I had seen books like these before, most memorably the copy that belonged to Hope Plantation.

I soon asked the librarian if she knew the original source for the books. After pulling the accession files she reported: 'This entire collection was donated to the Museum by one of its well-known and most generous donors.' Looking down her papers, she added: 'It appears that they were all purchased from a single source – from the firm of Henry Stevens and Son. The donor bought the remaining stock of this book from the Stevens firm when it went out of business and donated them all to the Museum.'[14]

I also soon learned that Henry Stevens and his sons and associates were not the only book-dealers who advertised that they could supply perfect copies of Harriot-de Bry to would-be buyers. The mania that took over Henry Stevens was shared by other renowned book-dealers as well. While researching the Harriot Papers at the British Library, I learned that the library owned one of two exceedingly rare maps that had been drawn by the Elizabethan alchemist John Dee (1527–1608) to prepare the way for the English exploration of the so-called New World. The companion map, said to have been used by Sir Humphrey Gilbert (Ralegh's half-brother) on his fatal voyage to America in 1583, was owned by the Free Library of Philadelphia, my own public library![15]

[14] Conversations with librarians at the National Maritime Museum, Greenwich, 23 December 2006.

[15] Shaffner, *William McIntire Elkins: With a check-list of his Americana, now in the Free Library of Philadelphia, by Howell J. Heaney* (Philadelphia, PA, 1956) p. 24 (item

How did a document so important in the origins of the British Empire come to rest in an American public library? That was something I hoped to learn when I returned to Philadelphia. The answer was fascinating and led me to a further understanding of how important copies of Harriot-de Bry came to be located in American libraries. When I went to see the companion map at the Free Library, I of course checked to see if the Library had copies of Harriot-de Bry. Sure enough, it had a complete set of the 1590 edition – Latin, German, French and English – one of the few complete language sets anywhere in the world. Neither the British Library nor the Bibliothèque nationale had a set. But my local library did! And this rare set, not listed in any of the standard guides to library holdings, came to the Free Library in the same manner as did the John Dee map. They were bought for William M. Elkins, an early twentieth century book collector who had donated his rare book collection to the Free Library,

Whereas the greatest Harriot-de Bry book-dealer in the nineteenth century was Henry Stevens, his successor in the first half of the twentieth century happened to have his world headquarters in downtown Philadelphia. Much like Stevens, he came to be one of the largest dealers in rare books for libraries and collectors across the USA. His moniker was Abraham Simon Wolf Rosenbach (1876–1952), the same man who helped Mrs Eleanor Elkins Widener of Philadelphia stock the bookshelves of the Widener Memorial Library at Harvard University. This cornerstone of the Harvard Library system was built as a memorial to her book-collecting son Harry Elkins Widener (1885–1912) who at the age of 27 had gone down on the *Titanic* on its fatal voyage from England to the USA in 1912.[16]

Rosenbach also made sure that Elkins's cousin William Elkins would not finish his book collecting until he had a complete set of Harriot-de Brys as well. In his notes on both the treasures from Petworth and on the four-language versions of Harriot-de Bry, he assured Elkins that he possessed the best versions money could buy. Like Stevens before him, he promised Elkins that he had become the proud owner of the one of the world's most 'perfect' copies of Harriot-de Bry. However, from my examination of the four-language variations of Harriot-de Bry that Rosenbach bought for Elkins, I could not find the same kind of 'perfecting' treatments that I had come to associate mainly with Henry Stevens.[17]

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42); also item 78 from the Leconfield Catalogue, pp. 33–4.

[16] Shaffner, *William Elkins*, pp. 12–13.

[17] Shaffner, *William Elkins*, pp. 3–17, esp. pp. 9–11 with details of the Leconfield and subsequent Herschel V. Jones sales of 1938, in which there were the 'perfect' copies of Harriot-de Brys that had belonged to Jones. In the 1928 Leconfield sale, Rosenbach also acquired for Elkins the enormously important manuscript history of Jamestown written by the ninth Earl's son George Percy, titled 'A Trewe Relatyon of the pr[oc]eedinge and occurrentes of Momente which have hap[pe]ned in Virginia from the Tyme Sr. Thomas Gates was Shippwrackte uppon the Bermudes Ano: Dni. 1612 [ca. 1625]' (Shaffner, *William Elkins*, p. 33).

Having seen what Dr Rosenbach had done for William Elkins, I decided to take a peek into the massive biography of Rosenbach which was written by two of his most devoted disciples – Edwin Wolf and John Fleming. I was curious as to whether, in addition to the names of Elkins and Widener, I might also find the name of John Workman Garrett, the creator of the Garrett Library at Johns Hopkins where I had seen my first complete language set of Harriot-de Brys. Sure enough, John Workman Garrett had become one of Rosenbach's most faithful customers and clients in the 1920s. By that time both Elkins and Garrett were bankrolling Rosenbach to buy all the treasures of early America he could turn up, including the complete sets of Harriot-de Bry that now reside in both of these once private libraries. [18]

After spending so many hours at Johns Hopkins, the University of North Carolina Chapel Hill, the Bibliothèque nationale, the British Library, the Bodleian Library, the National Maritime Museum and my own Free Library in Philadelphia, I felt that I could understand almost at a glance the probable history of virtually any copy of Harriot-de Bry that might come under my purview. I continued looking at copies, especially coloured copies, at the New York Public Library, the John Carter Brown Library in Providence, the Mariners' Museum in Newport News, VA, the University of Pennsylvania, the Bibliothèque centrale historique de la marine, the Vatican Library in Rome and the Library of Congress in Washington.

It was at the last of these – the Library of Congress – that I expected to have my horizons further expanded, since it possesses the fabulous Lessing J. Rosenwald Collection, said to be one of the greatest rare-book collections ever assembled in America. The Collection contains another of the four-language sets of Harriot-de Bry from 1590. Indeed, it is the English copy from the Rosenwald Collection that most people see when they pick up a copy of the inexpensive reprint of the book published by Dover Press in 1972. Since this reprint is so readily available to anyone who wants to see what a Harriot-de Bry is all about, I was sure that it would be the best one ever to be seen with human eyes.[19]

When I arrived at the Library of Congress, I asked to see all of the Rosenwald copies at once. But the Library of Congress, usually one of the most accessible and user-friendly libraries in the world, would not budge in this instance. As it turned out, however, a simultaneous examination was not necessary in this case, for, as soon as the librarian rolled out the cart with the Rosenwald copies on them, I hardly needed to look inside. From 20 feet away I could see and smell 'perfect' copies of the Harriot-de Bry. These were among the cleanest, most compact, most trimmed, most washed, most repaired and augmented and perfected copies

[18] Edwin Wolf 2nd and John F. Fleming, *Rosenbach. A biography* (Cleveland, OH, 1960), pp. 76–7, 224–5, 283, 290, 321 and 506.

[19] The Dover edition: Thomas Harriot, A *briefe and true report of the new found land of Virginia. The complete 1590 Theodor de Bry Edition*, with introduction by Paul Hulton (New York, 1972). The use of the Rosenwald copy is noted on the publication credits page and in a 'Publisher's note', p. iv.

of Harriot-de Bry I had seen. Although I took my usual obligatory notes on the English Rosenwald copy, I had little heart to continue with the other copies. I then knew that my survey of Harriot-de Bry editions was at an end. Seeing another 100 copies would not add to my basic store of knowledge about this rare and important book.

At this point I was ready to begin defining what was a 'perfect' copy of Harriot-de Bry, being completely mindful that in the process of examining quite a few editions of the work I had found two rival and totally averse notions of what was a perfect version.[20] Our idea of what is a perfect historical book has veered sharply from what Henry Stevens and to some extent Rosenbach and other book dealers and collectors had in mind in the nineteenth and early twentieth centuries. Whereas Stevens and Rosenbach wished to produce beautiful, complete, lush, clean and neatly packaged copies of the book, our original idea had been to find for our library at ECU a copy that was the least disturbed, the most pristine and most complete copy of the book we could find. By complete we meant that all of its current existing parts should be the parts that were in the book when it was first brought into existence in 1590 – no replacement pages, no major repairs, no special cleaning, no mixing and matching of printing stocks when the book was created. In short, just as original as the book could possibly be. We were not at all interested in a copy of the book that had gone through the hands of someone like Stevens or even those of the much less interventionist Rosenbach.

After having seen so many copies of the Harriot-de Bry, I was able to draw up a set of relatively objective measurements to apply to all of the copies of the Harriot-de Bry I had seen. I went back to those bits of data I had been collecting and essentially assigned values to various characteristics. The factors I rated the highest in determining which copies were closest to what we should now consider 'perfect' are highlighted in bold lettering below:

- Publication Year (**[must be] 1590)**.
- Language (**any [Latin, German, French or English])**.
- Edition (**first 1590**).
- Binding format (**stand-alone**, multi-volume).
- Page trim (**uncut** or **[lightly] trimmed**, retrimmed).
- Treatment of page edges (**plain**, gilt, marbleized).
- Page height and width (**in centimetres [the largest]**).
- Paper condition (**untouched**, very clean, clean, **dirty**, very dirty).
- Paper edges (pristine, **worn**).
- Paper treatment (**unwashed**, washed).
- Paper colour (white, **off-white**, dark, discoloured).
- Paper quality (**consistent**, varied).

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[20] I should note here that I am not including in these calculations those pristine or 'perfect' copies that came directly from the de Bry printshop into private hands. These are the 'perfect' versions that most research libraries would now like to possess.

- Paper replacements and repairs (**minimum).**
- Chain marks (presence and width in centimetres – **3 cm only**).
- Watermarks (**presence**, design, **[logical] distribution or periodicity**).
- Damages (water stains, worm-holes, torn and **[no]missing pages**).
- Printing (**clear**, faint, heavy inking).
- Titlepage (**presence**, **correctness**, condition**, printed** or tipped, **original** or substitute).
- Publication data plate (**clear**, worn, badly worn).
- Legibility of endings of crucial words (**'Theodori' and 'Feirabendii' [must be clear and complete]**).
- Dedication (**presence *and* correctness**).
- Headplates and footplates for book parts (what figures and images appear in each location **[must be present *and* standard]**).
- Adam and Eve engraving (**presence**).
- Plate I – 2 page map of Virginia (**presence**).
- Plates XIII and XVIII – 2 pages each (method of folding and insertion – **[must be a standard method of folding]**).

These included all of the characteristics we had discussed at ECU as being desirable in a copy of Harriot-de Bry that we might consider for purchase. It also included some of those that I considered important as I saw more and more copies of the book and formed a somewhat snobby but informed connoisseurship about Hariot-de Brys. But, most importantly, it excluded all those factors that might otherwise have led us to spend our money on a copy of the book that had been doctored in another age to be what Henry Stevens or one of his disciples deemed a 'perfect' copy of the book. How ironic that one of the fundamental books of Western history could have such different meanings to the curators and collectors of different eras!

With the assistance of Laureen P. Cantwell, a research associate from Drexel University, I began creating tables of cross tabulations and of crunched numbers to see what might be the results if I used my methods of evaluation. Ms Cantwell and I set out to determine as objectively as possible which book in our database came closest to our latter-day set of standards.[21] The winnowing process went something like this:

- Binding characteristics: 33 of the 38 books were single or stand-alone books, but only 16 of them had plain paper edges (not gilt or marbleized).
- Paper characteristics: 15 had untouched or dirty pages; 22 were unwashed; 20 had paper off-white in colour; and 19 had paper stock that was consistent from beginning to end.
- Watermark and chain marks: 29 of the 38 copies had the correct watermark (the fleur-de-lis) and had what seemed to be a standard distribution of

[21] I would like to express my appreciation to Ms Cantwell, who was completing her degree in library science at Drexel University at the time.

watermarks; 23 of the copies displayed the correct chain mark shadows of precisely 3 cm.

- Titlepages: 28 of the 38 copies had the correct titlepage; 22 of the 38 had relatively unworn title plates or title plates that had not been separately affixed to the page.
- Theodori and Feirabendii: of the 38 copies, 24 contained fully legible and unworn publication plates where the full term ‘Theodori’ could be read; 23 of them displayed the full term ‘Feirabendii’.
- Principal features: only 14 copies had all of the additional features (dedication, Adam and Eve, Plate I and Plate XIII) fully present, in a proper place or showing that they had not been substituted or modified in bindings.

By applying these standards somewhat rigorously, the number of copies of Harriot-de Bry that might be considered ideal or even close to perfect began to plummet. The fact of the matter is that many of these books have been badly handled, trimmed and cut, coloured by children, pages torn, saturated with water and subject to 100 other interventions that can occur over the space of four centuries. Those from the documented list of 38 books that emerged with some of the most critical ideal characteristics were the following:

Bibliothèque nationale – Arsenal	Latin and French
British Library	Latin
Brown University – Hay Library	Latin
Free Library – Elkins Collection	Latin and German
Harvard University – Houghton	French
Johns Hopkins – Garrett	French and English
Mariners’ Museum	Latin
University of Oxford – Bodleian	English (2)
UNC Chapel Hill – NC Collection	Latin and German
University of Pennsylvania	Latin

These Harriot-de Brys were mainly stand-alone volumes, had plain paper edges and correct and fully legible titlepages, displayed correct watermarks and chain marks, had paper consistency with unwashed off-white paper, had no substitute pages and were generally organized in what seemed a proper manner.

Before coming to a final conclusion on which were (according to my standards) the most perfect copies, I also took advantage of the page measurements I had made on all of the books. While one might assume a bit of serendipity and caprice in considering the size of a page as a factor in determining the historical worth of a book, such would not be true in the case of Harriot-de Brys. This is because every time there was an intervention with one of these books, there was a high likelihood that the book would be sliced and trimmed to bring a new uniformity to its pages. Or, as in the case of dealers like Henry Stevens, trimming might be a sure sign of

attempts to conceal the introduction of new pages or the resizing of pages through washing and cleaning.

As such, I was not surprised to see that most of the books that had already emerged as excellent copies through other measures now also cropped up as among the largest of the books I had seen. The largest of the books were as follows:

		Height (cm)	Width (cm)	Area (sq cm)
Bibliothèque nationale	Latin	34.4	24.2	832.48
Free Library, Philadelphia	Latin	34.5	23.8	821.10
Free Library, Philadelphia	German	35.0	23.4	819.00
University of Oxford – Bodleian	Latin	34.5	23.7	817.65
University of Pennsylvania	Latin	33.6	24.2	813.12
Mariners' Museum	Latin	33.0	25.5	808.50
UNC Chapel Hill	German	33.7	24.2	802.06
Bibliothèque nationale – Arsenal	Latin	33.6	23.8	799.68
Brown University – Hay	Latin	32.9	24.3	799.47
University of Oxford – Bodleian	English	33.6	23. 6	792.96

The correlation with other factors was not total, but it was close enough to prove to me that page size was something that should be given considerable weight in evaluating any particular Harriot-de Bry.

It was soon apparent that candidates to be considered ideal copies of Harriot-de Bry shared one other characteristic. None of them had been through the Henry Stevens system of therapeutic modification and perfecting. Some of them had come into place through the work of A.S.W. Rosenbach, but he had not modified the Harriot-de Brys he sold in anything like the manner of his predecessor. Even so, most of the copies that ended up near the top of my lists had been rendered into our present era from places far beyond the hands of these major dealers in early exploration literature.

I was finally ready to go back to my colleagues at ECU and tell them that the 1590 Latin edition we had so carefully examined and subsequently purchased was indeed nearly 'perfect'. I already knew, from my first and quite memorable comparison of the book with the copy from Hope Plantation, that 'Our Copy' definitely did not have any of the characteristics of one of those carefully 'perfected' versions of the book. The Hope Plantation copy, I would now guess, without any further documentary evidence, had gone through a Henry Stevens type of mill and had probably been bought directly from the Stevens firm or from someone who dealt with Stevens. Our Copy, by contrast, exhibited virtually all of the ideal characteristics of a little-modified edition. It was dirty and clearly had never been washed. It was, in fact, very nicely dirty in a way that you would want to see in an old book of this type. Its paper had the right watermark and the right chain mark spacing. There was a watermark on the titlepage and a good distribution of watermarks throughout the book. The paper was off-white and had a clear consistency throughout. All of its components were in a logical order. All

Table A.3 Copies of the 1590 edition analysed by the dimensions and total square area of their pages

Owner institution	Location	Language	Total page size (sq cm)	Page height (cm)	Page width (cm)
East Carolina University	Greenville, NC, USA	Latin	858.08	34.6	24.8
Bibliothèque nationale	Paris, France	Latin	832.48	34.4	24.2
Free Library	Philadelphia, PA, USA	Latin	821.1	34.5	23.8
Free Library	Philadelphia, PA, USA	German	819	35	23.4
Oxford University	Oxford, UK	Latin	817.65	34.5	23.7
University of Pennsylvania	Philadelphia, PA, USA	Latin	813.12	33.6	24.2
Mariners' Museum	Newport News, VA, USA	Latin	808.5	33	24.5
UNC Chapel Hill	Chapel Hill, NC, USA	German	802.06	33.7	23.8
Bibliothèque nationale	Paris, France	Latin	799.68	33.6	23.8
Brown University	Providence, RI, USA	Latin	799.47	32.9	24.3
Oxford University	Oxford, UK	English	794.4	33.1	24
British Library	London, UK	Latin	792.96	33.6	23.6
UNC Chapel Hill	Chapel Hill, NC, USA	Latin	782	34	23
National Maritime Museum	Greenwich, UK	Latin	781.56	33.4	23.4
National Maritime Museum	Greenwich, UK	Latin	776.88	33.2	23.4
National Maritime Museum	Greenwich, UK	Latin	776.88	33.2	23.4
National Maritime Museum	Greenwich, UK	Latin	776.44	32.9	23.6
National Maritime Museum	Greenwich, UK	Latin	775.5	33	23.5
National Maritime Museum	Greenwich, UK	Latin	773.56	33.2	23.3
Free Library	Philadelphia, PA, USA	English	773.5	32.5	23.8
Johns Hopkins University	Baltimore, MD, USA	Latin	770.5	33.5	23
Bibliothèque nationale	Paris, France	Latin	770.24	33.2	23.2

Owner institution	Location	Language	Total page size (sq cm)	Page height (cm)	Page width (cm)
Bibliothèque nationale	Paris, France	Latin	768.2	33.4	23
Bibliothèque nationale	Paris, France	Latin	768.2	33.4	23
Johns Hopkins University	Baltimore, MD, USA	English	763.75	32.5	23.5
Free Library	Philadelphia, PA, USA	French	762.72	33.6	22.7
Johns Hopkins University	Baltimore, MD, USA	French	762.3	33	23.1
Harvard University	Cambridge, MA, USA	French	759.24	33.3	22.8
British Library	London, UK	English	759	33	23
Bibliothèque nationale	Paris, France	Latin	758.64	32.7	23.2
Bibliothèque nationale	Paris, France	Latin	758.64	32.7	23.2
Bibliothèque nationale	Paris, France	French	758.64	32.7	23.2
Nat Maritime Museum	Greenwich, UK	Latin	754.4	32.8	23
Johns Hopkins University	Baltimore, MD, USA	German	749.1	33	22.7
Library of Congress	Washington, DC, USA	English	745.2	32.4	23
Oxford University	Oxford, UK	English	699.3	31.5	22.2
Princeton University	Princeton, NJ, USA	English	586.95	30.1	19.5

pieces were intact. While the book had been rebound in the twentieth century, the rebinding was a quite gentle process. All of the headplates and tailplates were in the right place. The titlepage was original, and the names Theodori and Feirabendii were clearly legible. There were repairs in the book, but they were minimal and had been done in a rather unobtrusive manner.

All of the pages were arranged in a correct and proper order. The edges of the pages were uneven due to ageing and handling. The pages had been just barely trimmed. There was no gilt or marbleizing on the page ends. All of the engravings were there and were clean and clear. The initial letters in the chapters and in the captions were those of the first Latin edition and first state of the book.

When it came to measuring the pages to determine how this book stacked up against other copies, I was in for a pleasant surprise. The height of the pages was 34.6 cm, one of the tallest of any Harriot-de Bry I had encountered. The width, at 24.8 cm, was greater than any other copy I had seen. When I multiplied these numbers, the page expanse was 858.08 sq cm, larger by 26 sq cm than any other copy examined. It was remarkable that this version of the book – the one that launched me into this project – could have survived all of the interventions and ravages that had faced most other copies of the book.

Certain that Our Copy could not have been through a Stevens programme based on its appearance and condition, I did some additional historical research on its provenance. We knew that the book had once belonged to another of those great private libraries that had been assembled in the nineteenth century. The book, on its titlepage, bore an embossed stamp indicating that it was once a part of the 'Sondley Library, Asheville, North Carolina'. The book had been one of the most precious of the 30,000 rare books that had been collected by the noted scholar and attorney Forster Alexander Sondley (1857–1931) of Asheville, who was a contemporary of William Elkins, Cyrus H. McCormick and John W. Garrett. But Sondley was much more of a scholar than were those heirs of great fortunes. He did not deal with big city book-dealers like Henry Stevens and A.S.C. Rosenbach. He bought his books from a variety of book-dealers who were able to produce just the book he wanted. His very impressive Latin copy of Harriot-de Bry came to him by way of a smaller book-dealer located in Lowell, MA, by the name of G.E. Merritt. And he bought his copy in the 1890s in its relatively untouched and unperfected form. By Sondley's standards, this somewhat dirty and well-handled book had just the pristine qualities he liked for all of his books.[22]

As I looked over the qualities of this former Sondley copy of Harriot-de Bry, I was ecstatic that I could report to my colleagues at Joyner Library and Thomas Harriot College that 'Our Copy' was probably one of the most perfect Harriot-de Brys in the world. But there was one tiny bothersome feature of the book that early in the process had come to be very troubling to me. Our Copy had a telltale

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[22] The information on Sondley is derived from the rather slim files that remain from the Sondley Collection that was once owned by the Pack Library of Asheville, NC. These files are in the North Carolina Collection of Pack Library.

worm-hole in it. The worm-hole was located in the upper right-hand corner of the pages of the book. Finding a worm-hole in a book of this age is nothing unusual, but the worm-hole in Our Copy was troubling because the worm had eaten through each and every page of the book from front to back, except for one page. How could a worm eat through every page of the book at precisely the same spot and somehow miss a single page halfway through the book?

Somewhat heavy of heart, I reported to my colleagues that I thought we had in hand probably one of the finest copies of Harriot-de Bry anywhere in existence – but how in the world could that hungry book worm have missed a single page? Perplexed by this one incongruity, we assembled our little group of cognoscenti to go through the book once again and to see if we could explain the missing wormhole. We went through the book page-by-page until we came to the offending page. Sure enough, there was no worm-hole. But, as we scratched our heads once again about this nettlesome mystery, one of our group – actually the new Dean of Thomas Harriot College, Alan White – said: 'Wait, there is a crease in that page. The page was folded when the worm went through the book.' Sure enough, there was a crease in the page – barely visible, but nevertheless there. The worm mystery thus melted away and our qualms about the 'perfect' character of the book were completely erased. This extraordinary copy of Harriot-de Bry is now the property of ECU and resides in the Special Collections Department of Joyner Library.

Whereas some institutions might be able to claim that they have *a* copy of Harriot-de Bry, others that they have *an excellent* copy of Harriot-de Bry and some others that they have '*perfected*' copies of Harriot-de Bry, we are pretty confident – based on all of these researches – that Our Copy is one of the most '*perfect*' copies of Harriot-de Bry on the planet! Few others can rival it.

Appendix B
Harriot's Latin

Charles Fantazzi

With his extraordinary skills as a mathematician and a master of astronomical navigation, Thomas Harriot was chosen by Sir Walter Ralegh to teach the science of navigation to his sea captains at Durham House in London. In addition to this scientific preparation, he had also learned the local languages of the Carolina region from two Algonquian Indians named Manteo and Wanchese, who had been taken to England in 1584 by the members of a reconnaissance expedition to the Carolina coast. To aid himself in this task, he devised a phonetic alphabet to represent the sounds of their language, perhaps the first of its kind in English. He may even have attempted to teach them English. Manteo accompanied both Harriot and the English artist named John White in another expedition in the following year. The two had been commissioned to produce a full natural history of the region, encompassing the people and their culture, its resources and the flora and fauna. The *A briefe and true report* was therefore an eye-witness account of daily life as it was lived by the native Algonquians. Unfortunately, having alienated the neighbouring Roanokes, the colonists had to leave hurriedly in Francis Drake's fleet in 1586, as a great hurricane was approaching, and the sailors had to throw most of the baggage overboard, including the specimens that Harriot had collected and all his notes, which he had intended to use in a more full-scale description of the new lands.

Harriot's *Briefe and true report* was first published in London in 1588 as a pamphlet, made up of six sheets of paper, each folded twice to make a small quarto or 48 pages.[1] It was a propagandist tract meant to refute adverse rumours about Ralegh's Virginia and to promote further colonization. Since such pamphlets were very rarely bound, they did not survive long, as is the case with this one. There are only six extant copies of this original document. The English clergyman Richard Hakluyt rescued Harriot's pamphlet for posterity, reprinting it together with accounts of previous expeditions in his *Principall navigations, voiages, traffics and discoveries of the English nation*, published in 1589. More significantly, he showed the work and John White's drawings to Theodor de Bry, a Flemish engraver and entrepreneur, who as a Protestant was forced to flee the Netherlands, which was then governed by the Catholic Spanish government. He fled to Germany but also spent some time in London, where he worked on maps and illustrations for a

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1 Cf. G.R. Batho, 'Thomas Harriot's manuscripts', in R. Fox (ed.) *Thomas Harriot. An Elizabethan man of science* (Aldershot, 2000), pp. 286–97 (p. 287).

book of sea charts. It was here that he met Hakluyt, who persuaded him to publish a far more elaborate and expensive edition for a readership of European aristocrats, merchants, scholars and connoisseurs. De Bry returned to Frankfurt-am-Main, centre of the international book market, to engrave, print and publish his edition. It was printed in four languages: Harriot's original English, French, German and Latin. It is a magnificent production, as may be seen immediately on the elaborate titlepage. The first words of the lengthy Latin title are more arresting than the original modest title: *Admiranda narratio, fida tamen, de commodis et incolarum ritibus Virginiae* (*An amazing but true account of the commodities and customs of the inhabitants of Virginia*). It was this Latin version, done by the renowned French botanist, Charles d'Escluse, usually referred to by his Latinized name Carolus Clusius, that became an international bestseller. This is not surprising, since Latin was the international scholarly language, whereas English at the time was a very marginal language that was not generally understood by scholars of other countries. It was not a fixed and settled language and exhibited great variations in grammar and orthography.

At this point I am prompted to interject a brief digression to explain the continued importance of Latin in the age of Harriot. It must be remembered that Latin remained the principal literary language of Europe all through the Middle Ages, and in the Renaissance it gained even greater impetus as the language of unquestioned prestige. By Harriot's time it had lasted for a millennium and a half, and there was no reason to doubt that it would last forever. Scientific works in particular were written in Latin up to the mid-eighteenth century and beyond, such as the great works of Galileo, Kepler, Copernicus and Vesalius. Galileo wrote the *Dialogo dei massimi sistemi* in the vernacular in order to reach a wider audience, but it was quickly translated into Latin three years later, in 1635, by Matthias Bernegger, a professor at the University of Strasbourg. The work had been condemned by the Holy Office a few years earlier, but copies of the Latin rapidly sold out in Frankfurt and Paris. Scientific works published in Elizabethan and Jacobean England are less important than those published on the Continent, but several significant works, since they were in Latin, were reprinted in Europe. For example, Robert Hues's *Tractatus de globis et eorum usu* (1594), in which Harriot collaborated, was reprinted in Amsterdam, Heidelberg and Frankfurt, William Gilbert's *De magnete* was first printed in London, but later in Frankfurt, and William Harvey's *Exercitatio anatomica de motu cordis*, on the circulation of the blood, was initially not printed in England at all, but appeared in Frankfurt in 1628, then in Venice, Leiden and Rotterdam, and not until 1653 in England.

Thus, it was natural that Harriot, when asked to write notes to explain the engravings of White's drawings, did so in Latin, a fluent, elegant humanistic Latin. Indeed, this contribution of Harriot to the de Bry Latin edition is much more polished than *A briefe and true report*, which he probably wrote in some haste. He was obviously proud of these luxurious de Bry volumes, for he mentions 'my discourse of Virginia in 4 languages' in some jottings on the back of the final folio

of a scientific work of his, ‘The Doctrine of Nauticale Triangles Compendious’.[2] As a graduate of Oxford, he had studied the rhetorical works, epistles and orations of Cicero, Aristotle’s *Rhetoric* and the usual canon of classical authors. It is interesting that we have in his own hand two ‘Supplicationes’ that he submitted to ‘the venerable congregation’,[3] one asking that he be exempted from an examination in which he presents evidence that he had completed four years of studies of dialectics and was therefore among those created *sophistae generales*, and the other requesting that he be graduated with only one disputation in Lent instead of the required two. Both supplications were approved, as we have records of this. He was also known to be a good Grecian and is mentioned specifically in the preface to *Chapman’s Homer* as being one of the poet’s two consultants, the other a friend of Harriot, Robert Hues. This is high commendation indeed.

Since the notes to the engravings are the only substantial piece of Harriot’s Latin presently available, I must base my remarks solely on those. There are some 4,000 folios of Harriot’s writings divided between Petworth House in Sussex and the British Library, but whatever exists in Latin must be chiefly scientific treatises whose specialized language I would not be able to decipher. Just recently, an important work of Harriot on the solving of algebraic equations has appeared in English translation.[4] This might have provided us with an example of Harriot’s Latin, but we cannot be certain how much is from Harriot’s hand and how much is a reworking or summary of what he wrote. The putative editor of this material, Walter Warner, an associate of Harriot’s in the household of Henry Percy, the ninth Earl of Northumberland, describes the work on the title page as ‘A treatise transcribed with the utmost accuracy and care from the last papers of Thomas Harriot, the celebrated philosopher and mathematician’.[5] What is noteworthy about the treatise is that his algebra was the first to be totally expressed in a purely symbolic notation, as Warner emphasizes in his ‘Preface to analysts’.[6] At the beginning of the treatise there is a series of definitions in which the language becomes more discursive, as in the definition of what is termed ‘specious logistic’, the expression of quantities by written signs. The writer, who in this case may well be Harriot, explains that he borrows the term ‘specious’ from commercial usage. But this is all conjecture and, what is more, I did not have access to the original Latin text, but had to rely on the translator. This would seem to accord well with his linguistic and navigational skills. Similarly, there may be vestiges of Harriot’s scientific writing in the fifth part of the *Tractatus de globis* of Robert

[2] D.B. Quinn and J.W. Shirley, ‘A contemporary list of Harriot references’, *Renaissance quarterly*, 22 (1969), 9–26 (11).

[3] University of Oxford Archives KK9, f. 296, quoted in J.W. Shirley, *Thomas Harriot. A biography* (Oxford, 1983), p. 54, note 22.

[4] M. Seltman and R. Goulding (eds), *Thomas Harriot’s Artis analyticae praxis* (New York, 2007).

[5] Ibid., p. 17.

[6] Ibid., p. 22.

Hues. It discusses the rhumb lines described on the terrestrial globe for the uses of navigation. The full Latin title is *De rumbis in terrestri globo delineatis, ubi eorum natura, origo et usus in navigandi ratione tractatur.* At the end of the book, Hues speaks with high expectations of a forthcoming treatise of Harriot on this subject.[7] The Latin of these writings is very clear and correct, but once again we cannot be at all certain that they are by Harriot.

In the English de Bry volume, Harriot's Latin captions were translated into English by Hakluyt rather competently, although like Harriot's English it is not always easily comprehensible. For that reason, a recent scholarly publication sponsored by the Mariners' Museum in Virginia[8] attempted to modernize Hakluyt's rendering of the captions and to offer a modern adaptation of Harriot's English in the *Briefe and true report*, in the latter case making some comparisons with the excellent Latin translation of Harriot done by Clusius. In all honesty it must be said that the attempt to modernize the English of the original report is most disappointing. It is neither modern nor archaic. The translation of Harriot's Latin captions is more respectable, but often Hakluyt's rendering is simpler and at the same time more accurate in its interpretation.

Harriot's first sentence, which describes the arrival of the English in Virginia, is a fine example of a Latin period, articulated by an exquisite prose rhythm. It must be read aloud to appreciate its sonorities:

> Virginiae maritima insulis abundant, quae difficilem admodum praebent in eam regionem aditum: nam licet frequentibus et laxis intervallis sint inter se discretae, quae commodum ingressum polliceri videtur, magno tamen nostro damno experti sumus vadosa esse et undarum brevibus infesta, nec unquam interiora penetrare potuimus, donec multis et variis locis minore navi periculum faceremus : aditum tandem inuenimus loco quodam nostris Anglis bene cognito.

Harriot displays a rich vocabulary in these descriptions, at times using rare words taken from lesser known classical writers. He was not an advocate of Ciceronianism, that is, the belief that only words and constructions of Cicero were admissible in the writing of Latin prose. There were followers of this movement in England at the time, although it was more of an Italian phenomenon.[9] To give but a few examples, he makes nice use of the quite rare adverb *decussatim*[10] (from the root *decem*, 10, which is written as an 'X' in Roman numerals, meaning 'so

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7 R. Hues, *De globis coelesti et terrestri ac eorum usu, conscriptus a Roberto Hues, denuo auctior et emendatior editus apud Aubrios et Schleichium* (Frankfurt, 1627), p. 258.

8 T. Harriot, *A briefe and true report of the new found land of Virginia*, facsimile edition accompanied by the modernized English text (Charlottesville, VA and London, 2007).

9 Cf. J.W. Binn, *Intellectual culture in Elizabethan and Jacobean England* (Leeds, 1990), pp. 270–90.

10 The word is found in classical Latin only in technical writers like Vitruvius and Columella.

as to produce the shape of an x' and therefore crosswise) to describe how the native women folded their arms when they went out in public. To indicate their tattoos, he cleverly adapts the Latin diminutive *punctiunculis*, literally 'little prickings'. For their walking about, he uses the word *deambulatio* taken from the play *Heautontimoroumenos* (*The Self-tormentor*) of Terence, a model of elegant style. To speak of the leading men of the villages, he does not hesitate to use a late Latin word, *magnates*, together with the more common designation, *proceres*. The first occurrence of this word seems to be in the Vulgate, at Judith 5:26 (*magnates Holifernis*), and frequently in Ecclesiasticus. In medieval Latin it became a rather common name for men closely associated with a ruling monarch, first in Hungary and then elsewhere. Describing their diet, Harriot again uses a rather rare generic word, *edulia*, 'things to eat'. In this same context, no doubt jocosely, he uses the Greek word *lastaurocacabus*, found only once in classical literature at Athenaeus, *Deipnosophists* 1.9c. It seems to have meant an aphrodisiac dish: the word *lastauros* in Greek means 'a kept boy' and *kakkabos* is the word for a cooking pot. Harriot compares it to the Spanish *olla podrida*. The Virginia facsimile edition comes up with the bizarre translation 'catamite stew', catamite being the Latin equivalent of the first part of the Greek word, but deriving originally from the Etruscan *Catmite*, which seems to be a corruption of the name of the Greek youth Ganymedes, whom Zeus had snatched up to Olympus to make his cupbearer. Hakluyt's *galliemaufre*, an old French word for a pot-pourri, is much more appropriate.

Harriot is not abashed to use the vocabulary of the more technical treatises of classical times, such as Cato's *De re rustica*, Virgil's *Georgics*, Varro or Pliny. Thus we find such words as *implexus*, 'an interlocking', used only by Pliny, and humble words like *scabere*, 'to scratch', *scrobis*, a word usually signifying a hole for planting trees but here denoting a kind of reservoir, as the word was used by Pliny, and *storea*, 'a mat'. The adjective *assus* (roasted) is common enough in classical Latin, but the verb *assare*, 'to roast', is found only in the late Latin writer Apuleius, noted for his idiosyncratic Latinity. For Indian corn, he uses the native word *mahiz*, Latinizing it as *mayzi grana.* At the same time he could use incisive phrases from the Roman writers: *irritamenta gulae*, 'the temptations of gluttony' (Sallust, *Jugurthine War*, 89.7); *genio indulgent*, 'have a good time' (Persius, *Satires*, 5.151). In comparing the earthen vessels (*figula vasa*) made by the native women to English brass cauldrons, he uses a Homeric word *lebes*, which Virgil, and Ovid after him, introduced into Latin. In only one instance, in my reckoning, does he use a word not found in other writers, *saturificio*, a rather strange formation.

On the page following the captions, de Bry himself probably intervened, informing the reader that the painter gave him five other images which he said he had found in an English chronicle. De Bry thought it was worthwhile to include them to show that at one time the inhabitants of Britain were no less savage than those of Virginia. He must have commissioned Harriot once more to provide brief descriptions of the Picts and Scots who are depicted there. At the end, Harriot, whom I presume to be the commentator as well, provides the reader with some

notes on this material, which demonstrate his familiarity with the classical authors. He quotes from Caesar's *Commentaries on the Gallic Wars*, in which Caesar gives a description of the Britons painted with a blue dye called woad in English.[11] Harriot's text differs in one particular from our present text in the word used for the dye. In our texts the word is *vitrum*, but he uses the word *glastum* (from Irish *glass*) and quotes the description of it in Pliny, 22.2. He also cites Herodian, who in narrating the campaigns of the Emperor Septimius Severus notes that the Caledonians aof Northern Britain went around naked to show off their tattoos.[12] It may be said, in conclusion, that in these descriptions of White's paintings, Harriot reveals an easy fluency in Latin, as would be expected of a man of his learning at that time.

[11] J. Caesar, *Commentaries on the Gallic Wars*, 5.14.

[12] Harriot, *A Briefe and true report*. The translator in the Mariners' Museum facsimile edition really goes astray here with this nonsensical rendering (*caveat lector*): 'Notwithstanding the Britains have lived with following Emperors, they seem to attack them of whom this picture is offered more greatly': ibid., p. 76. The Latin says simply that the Britons who lived under subsequent emperors (in this case Septimius Severus) seem to resemble more closely the people depicted in these images. Harriot's reference to Herodian is from *History of the Roman Empire since the death of Marcus Aurelius*, 3.14.7.

Figure A3 Copy, by Diccon Swan, of the portrait of an unknown man, possibly Thomas Harriot. The copy hangs in the hall of Oriel College. The original is in Trinity College, Oxford. By permission of Oriel College, Oxford

Appendix C
The Portrait of Thomas Harriot[1]

Diccon Swan

Some years ago the Provost of Oriel College asked me to paint a copy of the portrait, commonly said to be of Thomas Harriot, that hangs in the President's lodgings in Trinity College, Oxford. In undertaking the commission, I noticed several points of interest about the artist's approach. I record these points now, from my perspective as a portrait painter rather than an art historian or restorer. This is a case of one artist considering another after 400 years.

First of all, the portrait is painted on a board that is relatively rough and unsanded. The grain of the wood appears vertically through all the paintwork and adds a pleasantly primitive element of texture to the surface. It was presumably then painted with gesso (gypsum powder mixed with rabbitskin glue) to provide a painting surface, and the white gesso, dried but not sanded, was then painted with a thin glaze of skin-like colour to act as an underpainting for the portrait.

There seems to be a very restricted palette of colours used throughout the painting. Without the benefit of chemical analysis, I should say the colours are, or are the equivalent of, these five oil paints: lamp black, titanium white, raw umber, raw sienna and light red (or 'rouge anglais'). Everything in the painting is represented with these colours, mixed or pure. The basic flesh-coloured background appears to be a mixture of raw sienna mixed with light red; at least, when I used those colours to reproduce the colour that the artist used, I got a near-perfect match. The 'blue' that appears in the iris of the eyes is not actually blue at all, but black mixed with white, giving the appearance of blue. Similarly, although the ruff appears to have a blueish tinge, it is really only black mixed with white. The restricted palette gives the painting a nice unity, a colour-scheme of its own, and reminds one of a painting such as Goya's portrait of the Marquesa de la Solana in the Louvre, which appears to be multi-coloured but is actually painted entirely in black, white and red, with nothing else.

The artist's technique seemed to me to be particularly odd. Nowadays, when one paints an oil portrait, one paints the dark colours first, adding the lighter ones later and finishing with the lightest of all, usually the white flashes, such as the light in the eye. Here the painting starts with the flesh tone and any darkening is added later. The shading on Harriot's left cheek and left temple, for instance,

[1] Mr Swan's copy of the portrait now hangs in the Hall of Oriel College. His account is reproduced, with minor modifications, from the *Oriel College Record* (2007), by courtesy of the editor.

is achieved by stippling tiny touches of black paint on the pinky-brown of the skin and, in some places, smudging them, presumably by finger. This technique of painting dark over light is not entirely successful, but it does give the paintwork an illusion of depth as the layers remain separate and visible.

Some of the elements are very detailed and precise and are painted with tiny brushes: the eyelashes, the beard and the curls over his ears, for instance. It appears to be the technique of the miniaturist rather than that of a painter in oils. With water-based paints, one always paints dark over light, the way this artist does with his oils; painting transparent light paint over dark paint simply does not work. The second layer of paint can also wipe the first one off and cause muddiness and confusion. It is not beyond the bounds of possibility that this artist knew more about water-based miniature painting than larger-scale oil-painting. He may well have been trained as a miniaturist.

The eyes, beard and moustache are all very detailed and quite schematized (i.e. the hairs are painted in a pattern rather than being naturalistic), and the artist has attempted to paint the left ear in a detailed way too, stippling on black paint to create the illusion of the troughs in the ear. But it is very much less successful and appears unnatural. The hole in the ear has no depth and very much looks as if it has been painted on top of the skin colour, which it has. Some people say you can tell a good portraitist from a less good one by the way he paints the ears, and if this is the case, this artist has rather failed the test. However, the painting has considerable charm nonetheless.

The hair and the ruff are painted in much less detail, the one quite impressionistic and the other quite schematized. The doublet is painted in less detail still, with the pattern of the cloth painted in pure black randomly over the folds, not bothering to follow its undulations. And the background is by far the least careful part of the painting. There appears to be a raw umber wash with lamp black smears on it. It was a curious feature to reproduce as it had an obvious element of ineptness about it. But if the original is inept, the copy has to be artfully inept as well.

Maybe the least successful parts of the foreground are the areas around Harriot's left ear and around his hand holding the pomander. The left of Harriot's face appears to be in the shade, so the light source is clearly to his right. If this is the case, his neck below his ear would be in even more shade as it is further away from the light, but in fact it is painted as if it is as brightly lit as his forehead. The artist seems to have simply got it wrong. The hand is sketchily painted and the pomander looks more like a sponge.

I have a controversial contention that this painting is possibly a self-portrait, which, if true, would support doubts that have been raised by some as to whether it is, in fact, Thomas Harriot, there being no evidence, as far as I know, that he was a painter himself. It looks like a self-portrait. The position of the head, the look of the eyes and particularly the raised left shoulder towards the back of the painting all suggest a man looking at himself with his left arm raised (but invisible) to wield his brushes. It is curious that the left shoulder is raised when it is the shoulder

further away from us. Normally, if it were an ordinary portrait, it would be lower than the nearer shoulder.

This means that the artist would be left-handed. And this is consistent with the brushstrokes used in the face and in particular in the beard, where there is a very assured series of left-to-right diagonal strokes. A right-handed painter would be more adept at right-to-left diagonal strokes.

If it is a self-portrait, this would also explain why the visible ear appears rather flattened, as if viewed from the side rather than from the front. It looks as if the artist has unconsciously turned his head further to the right to see it better. But, admittedly, this is conjecture.

All in all, this painting is not the work of a great artist. It has failings in terms of plasticity and consistency of style, but it has a fresh and immediate charm, and there are passages of great delicacy and subtlety. The muted colour-scheme gives it a sophistication that outweighs the primitiveness of some of the painting, and the direct stare of the sitter gives it an intensity that compels us to look closely. Copying it was a fascinating job and, since it probably took me 10 times longer to copy than it took the artist to paint, I feel I probably know the painting better now than the artist himself ever did.

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Daniel Jon Mitchell

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